THE INSTITUTE OF PHYSICS

BULLETIN

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THE INSTITUTE OF PHYSICS

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J. J. Thomson*

By SIR GEORGE THOMSON, M.A., LL.D., Sc.D., Hon.F.Inst.P., F.R.S.

(*President of the Institute, Master of Corpus Christi College, Cambridge*)

DECEMBER 18, 1956, was the hundredth anniversary of the birth of Joseph John Thomson, who became Cavendish Professor of Experimental Physics and later Master of Trinity College, both at Cambridge. His best known work is the great share he took in the discovery of the electron, but he did much else, and besides his own contributions to science he was the originator, at least in this country, of research schools in physics, and for many years the leader of by far the greatest of them.

"J.J.", as he was generally called, is an example of a man who reached the profession in which he acquired fame, not by direct choice but by accident. He was originally intended for an engineer, and would have been enrolled as an apprentice, but that there was a long waiting list, so at the very early age of fourteen he went to Owen's College, in his home city of Manchester, which afterwards became Manchester University. Here besides engineering he studied mathematics, physics and chemistry, and when two years later his father died and the family resources became inadequate to pay the large premium then asked from an engineering apprentice, he stayed on at the College. He was lucky to get this training, for Owen's College was one of the few places where he would then have had any chance of practical work in physics, and even there it only really started a year after he came. Later he went up to Cambridge with a scholarship to Trinity, and read mathematics, which was a more normal approach in those days to the study of physics. He was second Wrangler in the Mathematical Tripos in the year that Larmor (known to physicists as the originator of the "Larmor precession") was senior. His early research was mathematical, and though he did some work in the newly founded Cavendish Laboratory, built by money supplied by another second Wrangler, the seventh Duke of Devonshire, he had been Cavendish professor for a good many years before his experimental work became as important as his theoretical. Physics in the 80's was in an odd state; it was in fact turning slowly over a dead centre, one impulse nearly exhausted the next not yet applied. The great advance of the mid-nineteenth century was due to the idea of energy, the impulse to come was the idea of electrons, quanta and nuclei, discrete units in fact as opposed to the earlier continuity. In those days atoms were a tool of the chemist, not too well thought of by the more philosophical of the physicists, who objected that there was no direct experimental evidence for them at all. "J.J." was to do much to supply it. He began by some work on the relation of generalized dynamics to physics and chemistry, in line with the classical ideas of energy but with an original turn of his own. Then he passed to the interpretation and the extension of the electrical theories of the first Cavendish professor, Clerk Maxwell. It is hard for a modern physicist, to whom Maxwell's equations seem even more fundamental and quite as obvious as Newton's laws of motion, to realize that when the theory first came out it was regarded as merely one competing formulation among many of the facts of electricity, and perhaps not the most attractive. The importance of the connection of electromagnetism and light seems not to have struck home, and the presentation was not easy to follow. In his first important paper on the subject "J.J." showed that an electrified sphere would have an extra mass as a result of its charge, or at least appear to do so, even if the charge itself weighed nothing. This idea of electromagnetic mass was the first hint of that relation between mass and energy which the coming of atomic bombs has made almost kindergarton knowledge: Einstein's form $E=mc^2$. Later "J.J." showed that the transport of energy in the electromagnetic field along the direction known as the Poynting vector, after one who was perhaps his greatest friend, is accompanied by a flow of momentum, which accounts for the pressure of light. "J.J." used in this theory the idea which goes back through Maxwell to Faraday, that "lines of force" such as those traced by iron filings near a magnet can be turned into "tubes of force" to have a measure of physical reality. It was a favourite idea of his which he worked on at intervals all his life. It is not in the fashion of modern physics, but the idea of "real" tubes of force has certain merits, and if one looks at the article in the *Reviews of Modern Physics* for July 1956, in which Dr. Post has set out the ideas that are guiding U.S. Government research into

* An address to the Midland Branch of the Institute, given in Birmingham on January 20, 1959.

thermonuclear actions, he will see how useful tubes of force can be. Later it caused "J.J." to put forward an idea which was a foretaste of Einstein's famous relation for light quanta ($E=h\nu$). "J.J." was led by considering the great concentration of energy on to certain privileged atoms when a gas is made conducting by X-rays to postulate a "speckled" wave-front for the radiation, the energy being concentrated and travelling along a few favoured lines.

A great expansion in Thomson's work came in 1896 : for more than a decade he had been working at the fascinating problems provided by the passage of electricity through gases, and the beautiful phenomena it causes. Up till then there had not been any very exciting results, but perhaps partly as a consequence of the discovery by Röntgen of X-rays in 1895 things started to move fast. "J.J." and Rutherford, who had just come to the Cavendish from New Zealand as a research student, showed, as did others independently, that X-rays make a gas conduct, and began to study the ions by means of which charges of electricity move through the gas. At the same time he was working on the properties of cathode rays. These are a feature of the electric discharge in a gas at low pressure which had been discovered in the middle of the century, and which in a slightly modified form are the working material of a television set. They can be seen as a glowing path leaving the negative pole of the discharge. When they reach the other side of the glass discharge tube they make it fluoresce. Lenard had shown that they could escape into the laboratory air through a very thin metal window and still produce a glow for quite a distance. He thought they were some sort of waves, as did Hertz who tried without success to deflect them by electric forces. That a magnet bent them easily had long been known ; they bent in fact as would a flexible wire carrying a current. Perrin had shown that when they entered a box they took with them a negative charge. Unlike their German colleagues, French and British physicists mostly thought them to be charged particles—possibly electrically-charged atoms, but then how did they get through Lenard's windows and why could Hertz not deflect them ?

"J.J." first improved on Perrin's experiment by showing that when the rays were deflected by a magnet they carried their charge round with them. Then he showed that the deflection of the rays by a magnet was the same in all gases and for several metal electrodes, if the voltage that drove the discharge was kept the same. From the start he was impressed by their universality. Then he came to measure their ratio of charge to mass. He was not the only person doing this at the time, and one German worker, Wiechert, produced an estimate, not strictly a measurement, a few months before his work appeared. Other measurements quickly followed, but the great merit of Thomson's two independent measures was that, unlike some others, they rested on well established physical principles and made the minimum of assumptions about the rays. The experiments really require the measurement of at least two quantities, the ratio of charge to mass called e/m and the velocity. Both these come in to each experiment, and experiments must be chosen so that they can be sorted out. In his first publication in 1897 "J.J." measured the magnetic bending and the heating effect of the same beam of rays. A few months later he used simultaneous electric and magnetic deflections, electric deflections you note, for he had explained Hertz's paradox of their apparent absence. In going through the rare gas in the discharge tube the rays make it electrically conducting in a way which neutralizes the applied electric force just where it is wanted. "J.J." discovered the reason and got over the difficulty by improving the vacuum.

The results showed, first that the rays behaved for these purposes like charged particles, next that if they were particles they must be at least 1000 times lighter than the lightest atom, hydrogen, unless indeed their charge were much greater than the charges carried by atoms in a gaseous solution which were believed, quite rightly, to be a natural unit. Then he measured the charge, not indeed on the actual cathode rays but on the ions that he had shown were produced by X-rays. It was an interesting method depending on the condensation of water on the ions which had been studied by C. T. R. Wilson. The charge was about what was believed to be that of an ion in solution—suggestive but not proof, since X-ray ions are not cathode rays. Finally in 1899 the logical chain was completed when Thomson measured *both* e/m and e for the *same* particles, namely those given out when ultraviolet light shines on a zinc plate. Both quantities were within experimental error the same as before. Masses at least a thousand times smaller than a hydrogen atom did exist, and were not limited to any particular material. In his first publication in 1897 "J.J." had emphasized his reasons for believing that his new particles—he called them "corpuscles" but I shall in future call them "electrons" —were indeed a universal constituent of all matter. If so, of course, there must be another, for all the electrons he found were negative and charges of the same sign repel, so matter made only of electrons would burst. The search for a positive constituent or constituents to bind matter together did indeed occupy him many years, but first there was much other work to do. This new conception of a common ingredient of matter, or perhaps more justly this new proof that an old conception was valid in changed form, started a fury of work throughout the world of physicists. Research students flocked to the Cavendish, and "J.J." built up a great school which was the seed bed for at least eight Nobel Prizemen in physics. It was probably the happiest time of his life, for though an individualist in his own research, he liked helping others and did it well. He enjoyed the radiation of their enthusiasm, which he had done much to kindle.

Of his own work in this period perhaps the most important was the calculations he made, based on three kinds of experiments, some done in the Cavendish, of the number of electrons in an atom. He had early guessed that it was nowhere in the thousands and that therefore only a small part of the total weight of an atom could be accounted for by the electrons it contained. The calculations showed that the number was of the order of the chemical atomic weight (e.g. 16 for

oxygen). It is actually roughly half this, but no high accuracy could be claimed for any of the methods.

To study the positive constituent of matter he turned back about 1905 to the gas-filled discharge tube. Positive rays, *canalstrahlen* in German, are seen as a luminosity in the gas behind if a hole is cut in the cathode. They had been known some time, and recently examined rather inconclusively by Wien in Munich. "J.J." showed that they were positively charged atoms or molecules, or at least had started as such. Nothing was found lighter than a hydrogen atom, or rather than a hydrogen atom less its one electron. The positive electrons that we know to-day are a very different matter and had to wait thirty years to be discovered. The robbed hydrogen atom was later to be called a proton and is, indeed, at least as far as we even now can tell, the particle Thomson was looking for. It was left for Rutherford to show how it, and the still undiscovered neutron, fit into the atom.

But the work on positive rays, though at first less spectacular, than that on the electron, was hardly less important in the end. Thomson early got the first real proof that atomic weight is a real thing in the sense that all the atoms of one kind have the same weight, as all half-crowns have the same purchasing power. Till then, a better analogy, for all anyone knew, might have been personal incomes, where indeed one can speak of an income per head in Britain and a different one in France, but without implying that all Britons or Frenchmen have precisely the incomes quoted. This point was settled because the atoms were graded into groups by mass, and the groups were seen to be sharply distinguished. But there is another possible analogy, the silver contents of a man's pocket. To take a slightly fantastic example, a man may go about with florins and half-crowns in one pocket, and shillings and sixpences in another ; if he jingles them about he is aware that the coins in the first pocket are in general heavy, those in the other light. He might even venture a guess that the first were three or four times as heavy. If he took them out and looked at them, he would see that each pocket held a mixture. So with atoms, those of a light element like hydrogen, may be a mixture of two kinds, both light, while those of a heavy element, say the gas neon, may also be a mixture of two kinds (it is actually three, but one kind is scarce), both heavy. Neon was in fact the element where this was first found. Shortly before World War I practically stopped work in the Cavendish, "J.J." published a paper with a statement that he was convinced that the gas neon contained atoms of two different species which behaved alike except for a difference in weight.

The proof was perhaps not quite complete, but after the war, Aston, who had assisted "J.J." in some of these experiments, proved it by the exercise of brilliant experimental talent, and of great determination when met by puzzling troubles. He proved moreover that neon is nearer the rule than the exception, and that most chemical elements contain mixtures of more than one atomic subspecies or *isotope*.

In 1917, Thomson was appointed Master of Trinity, and at the end of the war Rutherford succeeded him as Cavendish Professor. He kept a room in the laboratory and continued to do experimental work, chiefly on gas discharge. Of his later theoretical work the most important perhaps is that on the recombination of ions.

Though on the whole "J.J.'s" most important work was experimental, he had had most of his training as a mathematician. He was never good with his hands, depending a good deal on his mechanic assistant, E. Everett, and it was commonly said in the laboratory that it was just as well not to let him touch one's apparatus. He had the physician's gift of diagnosis, and could often tell a research student what was really the matter with an apparatus that the man had made and struggled with miserably for weeks. His own apparatus was simply designed and constructed without unnecessary refinement. The phrase "sealing-wax and string" with which a later generation describe the Cavendish apparatus of his day is an exaggeration of course, but not a great one, judged by modern standards at least there was a slightly amateurish air about it.

Thomson thought always in physical pictures, which he translated into mathematics, unlike some theoretical physicists for whom the mathematics comes first and suggest the physics. To some physicists, again, the apparatus precedes and the experiment follows ; to him as we have seen it was the other way round.

Yet this rather odd collection of glass and brass did in fact play a major part in producing the change in men's ideas of the nature of matter and energy that has occurred since 1896. Atomism now is the dominant idea ; not merely is matter made of discrete atoms in the technical sense of the word, but these atoms are made up each of a definite small number of electrons, protons and neutrons. In line with this, the quantum theory has shown that energy also occurs in discrete packets, while other whole numbers, quantum numbers as they are called, are fundamental in the structure of matter.

The Riddle of Relativity*

By Professor E. G. CULLWICK, O.B.E., M.A., D.Sc., M.I.E.E., F.R.S.E.
(*Queen's College, Dundee, St. Andrews University*)

Two years ago there started a controversy about relativity, in the pages of *Nature*, between Professors Dingle and McCrea[1] which has since reverberated around the globe. As is well-known, according to the Special Theory of Relativity a clock, which is moving relatively to the observer, should go more slowly than a stationary clock. If, therefore, a space ship carries a man at high speed out to the nearest star and back again, he should on his return find himself younger than his stay-at-home twin brother. Professor Dingle maintains that this conclusion is contrary to the principles of relativity, according to which all motion, including accelerated motion, is relative and has no absolute meaning.

On one thing Professor Dingle's critics are all agreed, that he is wrong. They do not all agree, however, on the nature of his error. Some give arguments which are no more than illustrations of the obvious fact that the reciprocal Lorentz transformation is algebraically consistent ; some claim that the problem requires the General Theory of Relativity ; and some appear to regard the matter as settled by their knowledge of four-dimensional space-time. Some argue with patience, while others thinly disguise their irritation. The matter has now, indeed, become international ; Britain and the United States are, of course, in it, while Builder[2] of Australia is severely critical of the General Theory addicts who are now supported by Contopoulos[3] of Greece. Contopoulos says :

> " We have seen that the arguments of Dingle and his followers are not right. How then is the controversy still continuing ? One reason is, I think, that many of Dingle's opponents are also wrong."

Among the latter, apparently, are McCrea, Darwin,[4] Fremlin[5] and Builder. Contopoulos considers Dingle's criticism of them to be justified since none gives a convincing argument for the asymmetry between the two clocks. He himself attributes this asymmetry to the velocities of the clocks *relative to the rest of the world.*

One is reminded a little of the battle of Arsûf, in the Third Crusade, when, led by Richard, the crusaders routed the infidel with much blood and satisfaction and then started to slay each other.

I propose to consider certain aspects of the problem from the viewpoint of an electrician. Not, alas, in the hope of giving you the final truth, but with the aim of clearing some of the battle dust away.

The special Theory of Relativity had its origin in Clerk Maxwell's theory of electromagnetism. According to Maxwell, electromagnetic waves in the absence of matter, travel with a definite velocity, c, in a universal medium known as the aether. Supposing the aether to be immobile, according to Newtonian principles the Earth's velocity through it should be superimposed on the velocity c, to give the velocity of light as measured in a terrestrial laboratory. The famous experiment of Michelson and Morley was designed to detect this effect, but none was observed. This null result supported the idea that the Earth must carry the surrounding aether with it, but this is inconsistent with well-known experiments on the aberration of light. Further, observations of double stars have been claimed by de Sitter to rule out the emission or ballistic theory of light as an alternative to Maxwell.

The first step in the solution of this dilemma was the suggestion by G. F. Fitzgerald that all bodies become contracted, in the direction of motion, when moving through the aether. If this contraction is in the ratio $(1-v^2/c^2)^{1/2}$ it was shown that this would explain the Michelson-Morley result. The major contribution, however, came from H. A. Lorentz. He showed that if one also introduces a time-measure, in a reference system moving with constant velocity through the aether, different from that in a stationary system, Maxwell's equations take exactly the same form in each of the two systems. Thus the velocity of light *in vacuo*, as deduced from Maxwell's equations, would be the same in each system and the Michelson-Morley experiment should not be expected to give a positive result.

By this means Lorentz deduced his famous " transformation." Suppose we have two inertial reference systems, S fixed in the aether and S' moving through it with velocity v. We suppose that there are two similar clocks, one stationary in S at the origin of co-ordinates and one similarly at rest in S'. The two clocks are set to read zero when the two origins are coincident, and we take the x-axis in the direction of the motion of S'. Then any physical " event " can be observed and measured in the stationary system S in terms of co-ordinates x, y, z and t, and according to Lorentz the same event is observed in S' in terms of co-ordinates x', y', z' and t' where

$$y' = y,\ z' = z,$$
$$x' = \beta(x - vt),\ t' = \beta(t - vx/c^2)$$

where $\beta = (1-v^2/c^2)^{-1/2}$.

It is then easily shown that if the velocity of light in S is measured and found to be c, the same will be true in S'. It also follows that a rod, parallel to the x-axis, and a clock, both stationary in S', will appear in S, in which they are moving, to be respectively contracted and reduced in rate in the ratio $1/\beta$. According to Lorentz, these affects are due to *motion through the aether.*

Lorentz's paper was published early in 1904,[6] and later that year Poincaré[7] proposed the *principle of relativity of inertial reference systems :*

> " physical laws, or the mathematical equations describing them, take the same general form in all reference-systems which are in uniform rectilinear motion relative to each other."

* A lecture given to the Scottish Branch of the Institute on November 4, 1958 (Dundee), November 10 (Edinburgh), November 11 (Glasgow)

Poincaré proposed this principle as a fundamental law of nature. It was already well known that it applies to Newtonian dynamics through the simple *Galilean* transformation :

$$x' = x - vt,\ y' = y,\ z' = z,\ t' = t,$$

but in order to make Maxwell's electromagnetic equations conform to it, it is necessary to adopt the more complicated transformation of Lorentz which reduces to the Galilean if v is very small compared with c.

This is the essential background to Einstein. In 1905 he published a paper—and the title is significant—" On the Electrodynamics of Moving Bodies,"[8] in which he developed the Lorentz transformation from two principles : the principles of relativity previously proposed by Poincaré and a new one of his own, the principle of constant light velocity :

> " the velocity of light, *in vacuo*, is the same in all inertial reference systems, and is independent of the velocity of the source of light in the reference system."

It may, at first sight, be difficult to see in what way Einstein introduced anything new. Maxwell's equations give a light-velocity c which is independent of the velocity of the source—this is of course natural in an aether theory—and Lorentz had shown what had to be done to make them conform to the principle of relativity of inertial systems. Therefore Einstein's new " principle " of constant light velocity was already contained in the work of Lorentz and Poincaré. Einstein's theory was, however, new in that he proposed a new interpretation. He implied that the Lorentz transformation transcends electromagnetism and embraces all natural phenomena, arising not from any supposed properties of the aether but just from the nature of things. He claimed that since it is impossible to measure velocity relative to the aether, the whole aether concept is meaningless. He also showed that it is meaningless to talk about the time at a distant place, or the simultaneity of two events at different places, unless we first define how we would make the necessary measurements to confirm our statements. He defined such measurements in terms of reflected light-signals, the velocity of light being specified to be equal to c in all inertial systems. He recognized that the *experimental* evidence for the value c depends on the *two-way* travel of a light signal, and, as we shall see later, that the acceptance of the same value on *both* legs of the light path is a matter of convention or deliberate choice. This choice he adopted as his " principle."

Let us now see how Einstein's viewpoint may be deduced directly from Lorentz's theory of effects due to motion through the aether. Suppose the system S to be at rest in the aether and that at time t_1, by the clock at O, a light ray is emitted by the observer O' towards O. Since the clock at O', moving through the aether, is going slowly this means that O' emits the ray, by his clock, at time $t_1' = t_1/\beta$. At time t_1, O' is at $x_1 = vt_1$ in S, so the ray reaches O at time

$$t_2 = t_1 + vt_1/c = t_1(1 + v/c) \qquad (1)$$

and at this instant O' is at $x_2 = vt_1(1 + v/c)$. The ray is reflected at O, and let T be the time taken for its return to O'. Then it travels a distance cT while O' moves a distance vT. Therefore

$$T(c - v) = vt_1(1 + v/c) \text{ and } T = \frac{vt_1(c + v)}{c(c - v)}$$

Hence the time in S at which the ray reaches O' is

$$t_3 = t_2 + T = \frac{t_1(c + v)}{c - v}$$

and by clock O' the ray returns at $t_3' = t_3/\beta$. We suppose that the reflected rays carries with it the reading t_2 of clock O. The reading of clock O' at time t_2 is actually t_2/β, so that if O' knows that the ray reaches O at time t_2/β by his clock he also knows that clock O, relative to his clock, is going fast. If so, he and O cannot regard their reference systems as being equivalent. But, in fact, O' cannot know that the ray reached O at S'-time t_2/β unless he knows his velocity through the aether, in which case he uses $c \pm v$ for the velocity of light in his system. Owing to the contraction of his rods and the slow rate of his clock, however, he cannot measure his velocity through the aether. Hence he cannot know that the ray reached O at time t_2/β by his clock.

So let us suppose that O' considers himself to be at rest in the aether. He will then use the constant value c for the velocity of light on both legs of its journey between emission and reception. He will therefore calculate the time, by his clock, at which the ray is reflected at O to be

$$t_2' = (t_1' + t_3')/2 = t_1\left(\frac{1 + v/c}{1 - v/c}\right)^{1/2} \qquad (2)$$

Now, from (1), this is equal to βt_2. So O' says that at time t_2', by his clock which he asserts is stationary in the aether and running at the correct rate, the clock O, *moving through the aether*, reads $t_2 = t_2'/\beta$ and is *slow*. Hence *each* observer can regard himself as being at rest in the aether, with his own clock running correctly and the other observer's clock running slowly.

This is the viewpoint of relativity. Each observer finds that Maxwell's equations are valid in his own system so that the velocity of light has the value c and the principle of relativity is fulfilled. It does not follow, however, that each observer *must* adopt Maxwell's equations, and the principle of constant light velocity, in his system, Each is equally justified, for the purpose of describing observed electromagnetic phenomena, in ascribing to himself any arbitrary velocity, less than c, through the aether. Each observer could, for example, regard the relatively moving observer as being at rest and himself moving, so that the relatively moving clock goes faster than his own. In synchronizing distant clocks stationary in his own inertial system, and on the x-axis, he would then use a light velocity $c \pm v$. Or he could regard the other observer and himself as having equal and opposing velocities through the aether, so that their clocks go at the same rate. In neither case, of course, would he obtain such a simple description of his observations, but this would not invalidate his theory. It follows that the principle of constant light velocity is no more than a convention which cannot be proved by experiment to be *necessary*.

It also follows that the slow rate of a moving clock is equally a convention, being subject to the condition that we first adopt the principle of constant light velocity. All that experiment can show is that, given the latter, the former may be true. This has in fact been confirmed by Ives and Stillwell,[9] for moving *electromagnetic* clocks—radiating hydrogen ions—with velocities up to about $c/140$, but the experiment is equally consistent with the interpretation that the velocity of light emitted by the moving ions is $c \pm v/2$ and that the frequency of the radiation is independent of velocity.

A further consequence is that the definition of *time* at distant points is also subject to convention. The accepted convention for the synchronization of distant stationary clocks is based on the principle of constant light velocity, and so is not *necessary*. The nearest star, Alpha Centauri, is some four light-years away. By convention we say that the date there at this moment is November 10, 1958, but it might equally well be anything between November 10, 1954, and November 10, 1962.

We have now seen that, so long as we are concerned only with the observations of two observers who remain permanently at rest in relatively moving inertial systems, Lorentz's theory is consistent with the principle of relativity and equivalent to Einstein's theory. If, however, one of the two observers changes from one inertial system to another, then the aether theory is no longer consistent with the principle of relativity.

Suppose that observer O remains permanently at rest in one inertial system, the Earth, and that O', a space-traveller, after moving away from O for a certain time at very high speed, reverses his motion and returns at the same speed to Earth. It is clear that O may regard himself as being at rest in the aether throughout. So if the duration of the outward journey of the traveller, as measured by O on Earth, is T, the earth-time for the round trip is $2T$. Since the traveller has moved, except for the short time taken to turn around, with constant velocity v through the aether, his clock has run slowly and will record a duration $2T/\beta$ for the round-trip. All physical processes in the space-ship are supposed to be slowed down in the same ratio, so that if O and O' are twins then O' will find that he is younger than O when they meet again.

From the aether viewpoint there is no paradox in this result, for the traveller can regard himself as stationary in the aether, with the Earth clock going slowly, on one leg of the journey only. On the other leg he must consider himself to be moving through the aether with a velocity greater than that of the Earth, so that the Earth clock is going faster than his own. Whatever velocity through the aether may be attributed to the Earth, calculation shows that the ratio of the readings of the two clocks, when they meet again, will be $1/\beta$.

The clock whose motion through the aether is non-uniform is slow compared with the uniformly moving clock in a ratio depending only on their relative velocity, as measured by either observer. Although neither observer can *measure* his velocity through the aether, it is clear that the asymmetry between the clocks must be *attributed* to motion through the aether.

It is also clear that this explanation is inconsistent with the principle of relativity, since the traveller can logically adopt the principle of constant light velocity, and Maxwell's equations, in one only of the two inertial systems in which he is successively at rest. If, therefore, the result is to be accepted as a true deduction from the theory of relativity, some valid justification must be found for banishing the aether concept and allowing the traveller to regard the Earth clock as going slowly *on both legs of his journey*.

From the aether viewpoint the traveller makes an appropriate change in the value of light velocity, relative to him, when he turns around, and consequently finds no discontinuity in the readings of the Earth clock as calculated from his observations. These, of course, he makes by means of light signals reflected at the Earth clock and returned to him bearing its reading. This reading does not correspond with the time at which the reflected ray returns to him, but with a previous time depending on his estimate of the velocity of light. If he uses the value c on *both* legs, however, he will deduce a discontinuity ; from being slow the Earth clock will suddenly become fast. If T/β is the duration of the outward journey as recorded by the traveller, he will observe the Earth clock to read $T(1-v^2/c^2)$ just before his turn-around and $T(1+v^2/c^2)$ just afterwards. Thus although the Earth clock is still observed by the traveller to be going slowly, it will be ahead of his clock when he returns to the Earth.

Now if it is a valid physical theory for him to use the principle of constant light velocity on both legs of his journey, as is required by the Special Theory of Relativity, it follows that there must be an equally valid physical theory to account for the sudden change in the reading of the Earth clock, as observed by him, when he turns round.

It is not sufficient, therefore, for relativists merely to show that no *physical* inconsistency arises in the accepted result. From our discussion from the aether viewpoint it should be clear that no physical inconsistency is involved, for a clock *may* run slowly owing to its motion through the aether. One can therefore calculate the rate and number of light flashes emitted and received by the two observers, using the relativistic Doppler formula, with complete confidence that the results will be consistent. Such exercises are, however, rather a waste of time since the reciprocal Lorentz transformation is an algebraic identity.

Now it is claimed that the explanation of the traveller's observations of the Earth clock, when he turns round, is completely provided by the General Theory of Relativity. For the traveller, during his acceleration, a uniform gravitational field is said to exist, throughout all space, and this makes the two clocks, as observed by the traveller, go at such different rates as to account completely for the sudden advance of the Earth clock. The necessity for such a theory is easily seen from the case of a clock attached to the rim of a rotating disc. An observer C at the centre of the disc, at rest in an inertial system, sees the clock in motion and therefore going slowly, but a second central observer C', rotating with the disc, sees the clock at rest but still going slowly. From the aether viewpoint there is no difficulty, since

the rotating observer knows that he is rotating in the aether and therefore knows that the clock at the rim is moving through the aether. In the theory of relativity, however, effects due to motion through the aether are denied and it is necessary to postulate an effect due to a difference of gravitational potential. This theory is a purely mathematical extension of the Special Theory to accelerated motion which, so far, has not been satisfactorily verified by experiment. Indeed, measurements of the spectral red-shift of the sun's light, that is the gravitational effect on the rate of an atomic clock, point to the possibility that the theory may be false.[10]

These considerations suggest, I think, the possibility that relativity theory may exceed the limits justified by established facts. The original purpose of the Lorentz transformation was to make Maxwell's electromagnetic aether-theory consistent with the principle of relativity, a principle which has never been contradicted by experiment. It is significant that the only experiments providing reliable confirmation of results specified by the Special Theory at high velocities concern elementary charged particles and atomic electromagnetic clocks, after these particles have been accelerated from rest. Take, for example, the well known variation of mass of an electron. In the classical experiments of Bucherer and Neumann, and others, the particles were first accelerated, and the same obviously applies to modern particle accelerators. Now it may be deduced from the Special Theory that energy should possess mass according to the law

$$m = /Wc^2,$$

but the same law may be deduced, for *electromagnetic* energy, by an analysis of Maxwell's equations and the use of Newton's Third Law of Motion.[11] If we then examine the acceleration of a charged particle by electromagnetic forces, and apply the mass-energy relation, we obtain at once the relativistic formula for mass variation.[12]

$$m = \beta m_o.$$

That is, the mass variation of an electron can be deduced quite independently of relativity theory. It thus appears that the Lorentz transformation has the property of enabling electromagnetic phenomena to be *specified*, which can also be deduced from more familiar physical principles. This is certainly true in the case of magnetic forces. For example, the Lorentz transformation applied to the known laws of electrostatics enables us to specify the force between current-carrying conductors which we usually regard as magnetic.

The same principle may hold in the case of the life of a meson moving with a very high velocity. It has been found by experiments on cosmic-ray mesons, assuming c to be the limiting velocity, that their life is consistent with the relativistic slowing of a moving clock. That is, it is greater than that of a slow meson. Their life is also found to be directly proportional to their kinetic energy. Now the reason for the decay of a meson is not known, but it is not unreasonable to suppose that its endurance in some way depends on its energy, to an extent *specified* by relativity theory. Again, in the case of the experiments by Ives and Stillwell on the light emitted by hydrogen canal rays, the particles were first accelerated and their mass thus increased by the energy given them. A physical change had taken place, during acceleration, and this may account for the frequency change specified by relativity.

It is important to note, that if an effect is to be *purely* a consequence of the Special Theory, it is necessary that the *same physical system* should be the object of observations made in *two* inertial systems. Now such a direct test of the theory has never been performed, and in these experiments the complete physical system, embracing both the accelerated particles and the accelerating source, is not the same after the particles have been accelerated as before. The experiments merely show that the properties of the particles, after acceleration, are different from their properties at low velocities or at rest. The view that they provide confirmation of the Special Theory, as distinct from relativistic electromagnetism, is based on the unproved assumption that the *rest-properties* of the particles are unaffected by the acceleration.

It thus appears that we may have confidence in the specification provided by the Special Theory for electromagnetic phenomena, without necessarily accepting the Lorentz transformation as providing a fundamental theory of space-time embracing all natural phenomena.

If, however, the Special Theory *is* true in such a general sense, then it is certainly necessary that Maxwell's equations should be beyond suspicion. For, if not, the Special Theory is built on an imperfect foundation. This, as we shall now see, is actually the case.

Maxwell, in developing his theory, considered very carefully the possible nature of the magnetic energy of an electric current. He regarded this energy as basically "electrokinetic," "since," he wrote, "we can form no conception of an electric current except as a kinetic phenomenon, its energy must be kinetic energy, that is to say, the energy which a moving body has in virtue of its motion."[13] As is well known, Maxwell regarded the medium or aether surrounding the current circuit as the seat of this kinetic energy, but before adopting this hypothesis he considered very thoroughly the question whether an electric current itself possesses momentum. It was necessary for him to resolve this problem since he built his field-theory on a dynamical analysis of current circuits by means of Lagrangian methods, from which the salient facts of electromagnetic induction and forces are deduced without specifying in any way the nature of the substance whose motion attends the flow of current and whose kinetic energy is taken to be the magnetic energy of the current.

The fundamental problem can be simply stated. Suppose unit length of a wire to have mass M_c, including the mass M_o of the substance whose motion in the wire comprises the current. Suppose also that the current is due to the motion, relative to the wire, of this substance with velocity v. Then the kinetic energy of this substance is identified with the magnetic energy, so that

$$\tfrac{1}{2} Li^2 = \tfrac{1}{2} M_o v^2$$

Now consider the wire to be moving with velocity w along its length, in the same direction as v. The mass M_o moves with velocity $v+w$ and the remainder,

$M_c - M_o$, with velocity w. The total kinetic energy is then

$$T = \tfrac{1}{2}(M_c - M_o)w^2 + \tfrac{1}{2}M_o(w+v)^2$$
$$= \tfrac{1}{2}M_c w^2 + M_o\, wv + \tfrac{1}{2}Li^2$$
$$= T_m + T_{me} + T_\epsilon$$

The term T_m is the kinetic energy of the whole mass arising from its velocity w, and T_ϵ is the magnetic energy when the wire is stationary. The term T_{me}, however, depends on both w and v and exists only when the current flow is combined with motion of the conductor.

On the other hand, suppose the substance whose mass is M_o, and whose kinetic energy comprises the magnetic energy, to be quite independent of the wire. The current is supposed to excite the motion of this substance in the surrounding aether, and for simplicity let us assume that its velocity has the single value v. Then when the wire moves through the aether the velocity of M_o remains unchanged and the total kinetic energy is merely

$$T = \tfrac{1}{2}M_c w^2 + \tfrac{1}{2}M_o v^2$$
$$= T_m + T_\epsilon$$

the term T_{me} being absent.

Thus the presence or absence of the term T_{me} depends on the basic nature of magnetic energy, and in an aether theory it should not appear. Maxwell therefore discussed the matter in detail and showed that, if T_{me} exists, three types of experimental effects should occur.

(*a*) If a circular coil is freely suspended by an axial thread with its axis vertical, any change in the current flowing in it should produce a rotation of the coil.

(*b*) A coil carrying current should exhibit gyroscopic effects and, if ferromagnetism is due to atomic current, the same should apply to a magnet.

(*c*) When a rapidly rotating coil, part of an unenergized closed conducting circuit, is suddenly stopped the inertia of the current carriers should cause a momentary current in the circuit.

Maxwell found no evidence for the existence of any of these effects, and he concluded with these words :

> " We have thus three methods of detecting the existence of the terms of the form T_{me}, none of which have hitherto led to any positive result. I have pointed them out with the greater care because it appears to me important that we should attain the greatest amount of certitude within our reach on a point bearing so strongly on the true nature of electricity. Since, however, no evidence has yet been obtained of such terms, I shall proceed on the assumption that they do not exist, or at least that they produce no sensible effect, an assumption which will considerably simplify our dynamical theory."[14]

Nothing could be clearer than this : the justification for Maxwell's development of his aether theory was his assumption that the term T_{me} does not exist. Nevertheless, all three effects have now been experimentally observed. The gyromagnetic effects were found by Barnett[15] in 1915 and by Einstein and de Haas[16] about the same time. In 1916 Tolman and Stewart[17] detected effect (*c*), while effect (*a*) was finally observed by Barnett[18] in 1930. All the effects are, of course, very small, and confirm that electrons are the current carriers in metals and the source of magnetism in iron.

These facts are all a matter of indisputable record, and show beyond the shadow of a doubt that Maxwell's aether theory of electromagnetism is incomplete. For these electron-inertia effects are *macroscopic* and according to Maxwell's theory should not occur. Not only must the theory be judged to be incomplete, but its logical foundation, as a theory of electromagnetic effects in a material medium or aether, is denied by experiment.

This being so, the Lorentz transformation appears in a different light. Instead of expressing fundamental truths, transcending electromagnetism, about space-time, or even of describing the effects of motion through the aether, it appears as a necessary corrective of the grosser failings of Maxwell's theory. Although, under carefully prescribed conditions, it can hide the aether, it cannot put electrons into the theory. Nevertheless, it seems to have the power of specifying hitherto unsuspected electromagnetic effects for which, as yet, we have no satisfactory physical explanation. It should now be obvious that its relevance to the rate of ageing of a space traveller is, to say the least, doubtful.

This viewpoint is, of course, quite contrary to orthodox doctrine. Relativity is taught as a general theory *transcending* electromagnetism, and a typical example of the usual presentation is to be found in a modern textbook on atomic physics.[19] The author derives the Lorentz transformation from an analysis of the Michelson-Morley experiment. The observer for whom the apparatus is stationary receives the two reflected light signals simultaneously, and a moving observer works out the necessary ratio of the lengths of the two arms so that he, using the principle of constant light velocity, agrees that the two rays arrive simultaneously at the eye-piece. To do this, he must of course assume that the velocity of light, relative to him, is equal to c even when reflected from a moving mirror. That is, he assumes that Maxwell's theory of aether transmission holds in his reference system. In spite of this, the author claims that he has derived the transformation *mechanically*,[20] and that *it is not merely an electromagnetic property*. He then goes on to state that the transformation leads to physical conclusions which hold " for *all matter* since they have been derived in terms of dynamics only, *without invoking any electromagnetic properties* as postulated by Lorentz." This sweeping assertion is thus based on an argument which is clearly false and which denies the true foundation of the Lorentz transformation.

Lorentz himself was careful to say that he put forward his theory with all due reserve.[21] In contrast, Einstein regarded his interpretation as " incomparably more satisfactory " than that of Lorentz,[22] and stated " the special theory of relativity has rendered the Maxwell-Lorentz theory so plausible, that the latter would have been generally accepted by physicists even if experiment had decided less unequivocally in its favour."[23]

Einstein thus claimed that the Special Theory is more fundamental than the electromagnetic theory of Maxwell and Lorentz, and that the latter is correct because it fulfils the requirements of the Special Theory.

This is an extraordinary claim. Einstein's development of the Lorentz transformation is based on a so-called principle which is, in fact, a deduction from Maxwell's aether theory, namely that the velocity of light, *in vacuo*, is independent of the velocity of the source or the reflector. It is nonsense to state that this principle is purely mechanical and independent of electromagnetic theory, for it is an essential property of medium-transmission. The aether concept cannot be abolished by mere words. The Special Theory is in fact completely contained in the Lorentz transformation, which is the algebraic solution of the problem of making Maxwell's theory conform to the principle of relativity of inertial systems. It is the latter which is the really fundamental principle. If we accept it *and* Maxwell's theory, we are led at once to the Special Theory. How, then, can the Special Theory make Maxwell's theory plausible ? Given one, the other follows. The claim is an excellent example of the logical fallacy of the circular argument.[24]

The truth seems to be that this principle—light velocity independent of velocity of source or mirror—is the basic *dogma* of the Special Theory. If you are a true relativist you believe that it *must* be so, independently of any particular theory of electromagnetism. You must accept it as an axiom never to be questioned, for without it Minkowski's beautiful theory of space-time, in which space by itself and time by itself fade away into mere shadows, loses its foundation. It also follows, of course, that you must not doubt Maxwell's equations, for they are guaranteed by the Special Theory. But here, in this principle, we have something quite different from the convention that the to-and-fro velocity of light, when emitted by a stationary source and reflected by a stationary mirror, is taken to be constant. We have a statement about the physical behaviour of light that can be tested, given a perfect vacuum, by experiment.

Now in addition to the argument of de Sitter[25] about the light from double stars we have the record of certain laboratory experiments with moving mirrors and sources, such as those of Michelson[26] and Majorana.[27] These experiments were limited to velocities, of source or mirror, very small indeed compared with c, but they are certainly *consistent* with the conclusion that the light velocity, over a to-and-fro path, was constant.

Relativists consequently claim that the experiments, together with de Sitter's argument, prove the truth of the principle. They seem to overlook the fact, however, that the experiments were not performed in a vacuum, let alone a perfect one. Suppose that light is propagated not by aether transmission but by the travel of photons: then surely a reliable test of the principle requires a completely free path, devoid of matter, between source and receiver. This condition is not fulfilled even in the travel of light from a star to the earth, so it is evident that the principle of constant light velocity *in vacuo* is *proved* neither by these experiments nor by observations of double stars.

It seems to me to be unfortunate, and even dangerous, that an important scientific theory should be taught with so little regard to the true discipline of science. It is said that it is better to travel hopefully than to arrive, and the dogma that we have arrived at the truth is not conducive to further mental effort. It may be that certain human minds are powerful enough to comprehend all things, and to uncover the secrets of the Universe by pure thought. On the other hand, the ultimate truths of nature may be beyond human reach, but it is the business of science to keep searching, without blinkers of its own making. Even if, in the end, the Preacher may be found to be right.

> " Then I beheld all the work of God, that a man cannot find out the work that is done under the sun ; because though a man labour to seek it out, yet he shall not find it ; yea further, though a wise man think to know it, yet shall he not be able to find it."[28]

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27. Q. Majorana, *Phil. Mag.* (6th Series), **35** (1918), p. 163.
28. *Eccles.*, **viii,** 17.

Notes and News

South Western Branch

The date of the Summer Meeting of the Branch at Shrivenham when Dr. C. E. Wynn-Williams is to give a talk on "An introduction to electrical logic" has been changed, and will now be held on June 26, not June 19 as previously announced.

Stress Analysis Group meeting

The meeting of the Stress Analysis Group in April on "Strain measurement at elevated temperatures" has been cancelled.

Australian conference on solid state physics

The Australian Branch of the Institute of Physics announces that it is arranging a conference on "Solid state physics" to be held from August 17-24, 1959, in Melbourne. There will be a number of overseas visitors attending the conference.

Details may be obtained from the convenor of the organizing committee, Dr. W. Boas, C.S.I.R.O., Tribophysics Laboratory, University of Melbourne, Carlton, N.3, Victoria, Australia.

The Physical Society—two symposia

The Acoustics Group of the Physical Society is sponsoring two symposia dealing with the problem of noise and vibration. The first, at 2.15 p.m. on March 24 in the Physics Department, Imperial College, S.W.7, under the title "Recent studies of noise problems" will survey recent work on the measurement and effects of noise, with contributions on certain aspects of aircraft and automobile noise. It is open to all without formality. The second is a one-day meeting on April 7 in the Physics Department, University of Southampton. Entitled "New techniques in the analysis of noise and vibration," it will include contributions from both practical and theoretical aspects on the use of correlation techniques, of digital computors for data processing and analysis, and on general applications of statistical communication theory. It is a joint meeting with the Institute of Physics. Further details may be obtained from Mr. D. M. A. Mercer, Physics Department, Southampton University.

International conference on information processing

An international conference on information processing will be held in Paris from June 15 to 20, 1959. The conference is being organized by the United Nations Educational, Scientific and Cultural Organization (U.N.E.S.C.O.) with the help of a group of consultants from various countries. The program of the conference will comprise six main headings: (1) Methods of digital computing (including numerical analysis); (2) Logical design of digital computers; (3) Common symbolic language for digital computers; (4) Automatic translation of languages; (5) Collection, storage, and retrieval of information; (6) Patterns recognition and machine learning. Details may be obtained from: The Honorary Secretary, Group B—Computation and Automatic Control, British Conference on Automation and Computation, c/o The Institution of Electrical Engineers, Savoy Place, London, W.C.2.

Symposium on molecular structure and spectroscopy

Symposium on molecular structure and spectroscopy will be held at the Department of Physics and Astronomy, The Ohio State University, this year from June 15 to June 19, 1959. There will be discussions of the interpretation of molecular spectroscopic data as well as methods for obtaining such data. In addition, there will be sessions devoted to those phases of spectroscopy of current interest. Further information is available from Professor R. A. Oetjen, Department of Physics and Astronomy, The Ohio State University, Columbus 10, Ohio.

Symposium on interferometry

An international symposium on interferometry is to be held at the National Physical Laboratory, Teddington, Middlesex, on June 9, 10, 11, 1959. The symposium is one for specialists and will not deal primarily with technical applications. There will be four half-day sessions devoted to the presentation of papers and to discussion, and a fifth session for the viewing of demonstrations.

Attendance is by invitation, and applications should be made to Mr. L. H. McDermott at the National Physical Laboratory.

Members are reminded that applications for tickets for the Annual Dinner, at the Park Lane Hotel, Piccadilly, London, on May 5, 1959, should be sent to the Secretary by April 10. Tickets (exclusive of wines) are 40s. each, but Graduates under the age of 25 years, and Students of the Institute may have one or two tickets at 30s. each.

Correspondence

The function of a university physics department

It was delightful to read Prof. Lipson on "The function of a university physics department." It is good to see an academic man looking at his own work. Too often the plea for academic freedom has been extended to freedom from reasonable self-criticism. On the whole the article was very sound.

There are two points I would like to amplify both from my own experience. First, I would go further than Prof. Lipson in speaking of the value of teaching to research. He seems to imply that it is only in teaching one's own research speciality, that research can benefit. But nowadays significant advance comes again and again from the bringing together of two apparently unrelated branches of physics and developing their relationship. There is no better way of achieving this than for a man to have his own research project present in one half of his mind, and be forced to pass the whole of physics (however at first unrelated) through the other half once a year while giving a general physics course. It is not only good to have to explain your research to others (hence colloquia), it is good to be reminded of the Theorem of Parallel Axes, and pyro-electricity and all the rest of it. Very often a hold-up in a research program has been resolved by the mere reminder of a branch of physics otherwise overlooked.

Indeed, it has been a continual source of wonder to me that large industrial research establishments have functioned, apparently quite well, without the stimulus of regular teaching. If one were organized on the basis that *everybody* went out half a day a week for at least one term per year and taught an elementary class at a local technical college, I believe there would result an improvement in the quality of their *research*, which would more than compensate for the time, at first sight, lost in the process. It would also help materially to overcome the present lack of effective contact between industry and our technical colleges.

The second point covers the value of research to the teacher. I am still near enough to my student days to have a clear memory of the classes I attended given by many distinguished men at Cambridge. Two of the most effective teachers were Alex Wood and Dr. G. F. C. Searle. Alex Wood was still doing consultative work in acoustics, but apart from that, neither had done "research" as we now understand it, for nearly twenty years. But neither of them was any the less lively in his approach for that, nor were Wood's lectures on mechanics and light (on which he was *not* researching) any worse than his lectures on sound. It is clear, therefore, that a university teacher only requires (*a*) to have done research in the past, to recognize its flavour and (*b*) to be in day-to-day contact with people who are doing research—even if the latter (like one F.R.S. I remember) would be quite incapable of getting anything across to a class.

It would be a pity if a man with something of Searle's gift for designing new experiments, and a little research experience in the past, was denied a place on a university staff because his present interests were in devising new methods of teaching, rather than of splitting the atom. It is time we recognized a vocation for teaching and saw it was not necessarily made subservient to research.

G. L. Rogers (*Fellow*).

College of Technology,
Birmingham, 4.

The teaching of magnetism and electricity

There has recently been a certain amount of discussion of the theory of magnetism and electricity following Professor Lipson's challenge, in the *Bulletin* of January, 1957, for the development of a satisfactory field theory. In his article on "Electrodynamics" in the *Bulletin* of December, 1958, Dr. Builder attempted to achieve this by deriving, from Coulomb's Law and the force transformation of relativistic mechanics, the well-known Grassman equation for the force $\delta\mathbf{F}^1$ exerted on the current element $i^1\mathbf{dS}^1$ by the element $i\mathbf{ds}$:—

$$\delta\mathbf{F}^1 = \frac{\mu_o}{4\pi r^2}\, i\, i'\, \mathbf{dS}^1 \times (\mathbf{dS} \times \hat{\mathbf{r}}) \qquad (1)$$

He also quoted the analogous Ampère formula in the form:—

$$\delta\mathbf{F}^1 = \frac{-\mu_o}{4\pi r^2}\, i\, i' \left\{ \mathbf{dS} \,.\, \mathbf{dS}^1 - \tfrac{3}{2}\, \mathbf{dS} \,.\, \hat{\mathbf{r}}\; \mathbf{dS}^1 \,.\, \hat{\mathbf{r}} \right\} \hat{\mathbf{r}}, \quad (2)$$

and pointed out that this result is incompatible with the special theory of relativity. However, he also mentioned, but without emphasis, that if equation (2) is integrated around a closed circuit (to obtain the force exerted by a closed circuit on a current element) then it is algebraically identical to the integral of equation (1), i.e.:—

$$\delta\mathbf{F}^1 = \frac{-\mu_o}{4\pi} \oint_S \frac{i\, i'}{r^2} \left\{ \mathbf{dS} \,.\, \mathbf{dS}^1 - \tfrac{3}{2}\, \mathbf{dS} \,.\, \hat{\mathbf{r}}\; \mathbf{dS}^1 \,.\, \hat{\mathbf{r}} \right\} \hat{\mathbf{r}}$$

$$= \frac{\mu_o}{4\pi} \oint_S \frac{i\, i'}{r^2}\, \mathbf{dS}^1 \times (\mathbf{dS} \times \hat{\mathbf{r}}) \qquad (3)$$

In other words, in its integrated form, the Ampère formula becomes the same as the Grassman, and therefore satisfies the requirements of relativity.

Bearing this in mind, the purpose of this letter is to submit one or two observations on the part played by magnetism and electricity in the education of the professional physicist. Theoretical electromagnetism constitutes an important body of knowledge, yet many students fail to appreciate the experimental evidence upon which it is founded. This failure tends to be aggravated by certain textbooks written from what is often accepted as the conventional M.K.S. approach in which equation (1) is put forward almost in the form of a hypothesis to be justified *a posteriori*. While this may be satisfactory from a purely mathematical point of view its avoidance of a controversial topic in physics is hardly to be commended.

Now, it is well known that the development of *electrostatic* theory can be based on the Cavendish experiment. Accepting the null result (that there is no charge on the inner sphere) the inverse square law of

force between charges can be deduced on making certain assumptions. This derivation can be discussed in detail with the student, as illustrating the general technique of deriving a physical law from a crucial observation. This is an important aspect of his education.

Similarly, the formulae of *electrodynamics* ought to result from an analogous consideration of the fundamental experiments of Ampère. Possibly opinions differ here as to the precise method of deduction. Nevertheless, since the experiments involved *closed circuits* it seems inevitable that an integrated equation will be obtained. This is usually quoted in either of the two equivalent forms of equation (3), involving only a single integration. However, it is perhaps worth noting that if equation (3) is integrated again (with respect to S^1), then it is easily shown that the force $\mathbf{F}^1$ exerted by one closed circuit upon another closed circuit is given by:—

$$\mathbf{F}^1 = \frac{-\mu_o\, i\, i'}{4\pi} \oint\oint \frac{(\mathbf{dS} \cdot \mathbf{dS}^1)\,\hat{\mathbf{r}}}{r^2} \qquad (4)$$

This result, therefore, is both relativistically satisfactory, and symmetrical with respect to the two circuits.

But equation (3) as it stands, permits the introduction of the concept of the magnetic field of the closed circuit S (around which the single integration has been performed). This, in turn, leads to the following field equations:—

$$\text{curl } \mathbf{H} = \mathbf{j} \qquad (5)$$

$$\text{curl } \mathbf{E} = -\frac{\partial \mathbf{B}}{\partial t} \qquad (6)$$

There is an obvious lack of symmetry here. The missing term involving $\partial\mathbf{D}/\partial t$ has been overlooked because equation (3) applies *only to closed circuits*. And, of course, if the theory is extended to open circuits by introducing Maxwell's displacement current, then the modified field equations provide a description of the propagation of electromagnetic waves. An open circuit is one containing the equivalent of a condenser, together with an inevitable inductance and resistance, in which an *oscillatory* current can be maintained (whereas the unmodified equation (5) only holds for *steady* currents). Consequently, the generation of electromagnetic waves can be related to the *acceleration* of electric charges. Hence, it seems necessary to insist that the Ampère-Grassman formula should be used only in the integrated form of equations (3) or (4) in order to demonstrate to the student that an apparently minor pedantry later turns out to be of fundamental importance. This procedure also avoids certain anomalies (such as the apparent violation of the conservation of momentum) which invariably arise when attention is confined to equations (1) and (2).

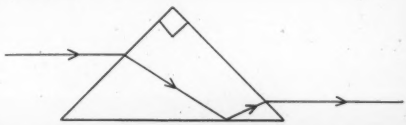
Furthermore, it is certainly not at variance with Dr. Builder's conclusion, on other grounds, that equation (2) must be rejected, while equation (1) may be retained. We are suggesting that, in the present context, equations (3) and (4) are, in fact, the appropriate ones, particularly from the teaching point of view because they enable theoretical electromagnetism to be presented to the student in the self-consistent manner outlined above. Then at a later stage when the theory of relativity is introduced, Dr. Builder's relativistic derivation of equations (1) and (3) well may be of considerable value.

F. J. Pearson (*Associate*).

College of Technology,
Birmingham, 4.

0° or 360° ?

I am sure everyone is aware of the simple use of a right-angled isosceles prism as an erecting prism—i.e. when the incident ray of light strikes one of the shorter faces, is refracted on to the hypotenuse, totally internally reflected, and finally emerges from the third face parallel to its original direction (as in the diagram).



In the Oxford and Cambridge Schools Examination Board G.C.E. " O " Level Physics I paper of December, 1958, the following question appeared:—

" Calculate the critical angle for glass of refractive index 1·5. Hence show how a right-angled isosceles glass prism of refractive index 1·5 can be used to turn a ray of light through an angle of (*a*) 180°, (*b*) 360°. Draw diagrams to illustrate your answer."

I shall be interested to hear of any views members of the Institute may have on whether a ray of light is turned through 360° when it passes through a prism as indicated above, or whether, in fact, the ray suffers no deviation whatsoever.

D. F. Perrens (*Associate*).

Eastbourne College, Eastbourne.

[*The examiner who set the question referred to has been invited to comment upon the above letter, but no reply has been received.*—Ed.]

The fallacy in physics

Dr. Maude has produced quite an intriguing puzzle in the December *Bulletin*, regarding the reflection of a train of plane waves by a moving conducting surface, but there appears to be a simple fallacy in his argument. For he assumes that the amplitude must remain unaltered, whereas when the conductor moves through the magnetic field component of the incident radiation it will act as an elementary generator : there will be an e.m.f. induced in it in addition to the electric field of the radiation : this will increase the induced current and consequently the amplitude of the reflected wave.

The increased energy must of course be the work done in moving the conductor against the " pressure " of the incident radiation, and this does involve one of the ultimate problems—if not *the* ultimate problem—of electrodynamics, i.e. *how* can a transverse field produce a sideways push ? It does, of course—after all, generators

work, and motors work—but *HOW* ? " It don't seem natural, somehow." I must say, the more I think about it the less I like it.

M. B. Leitch.

43 Belstane Road, Carluke, Lanarkshire.

I was very interested in Dr. Maude's article " The fallacy in physics and mathematics " in the December *Bulletin*. The fallacy in the proof that radiation pressure acts towards the source is to be found in the statements " If the plate is a perfect conductor no electric field can exist in it . . . . This conclusion holds if the reflector is moving with a velocity v in the positive direction of x." The latter statement is true only if the field is referred to an observer who is stationary with respect to the plate, whereas the rest of the argument relates to an observer who is stationary with respect to the co-ordinate system.

If $v^2 << c^2$ so that relativistic effects may be neglected, the correct result is obtained as follows :—

Let the plate intersect the x axis at $0'$ and let primes be used to signify quantities referred to an observer at $0'$.

Let E_i and H_i be the amplitudes of the electric and magnetic components of the incident wave in absolute units referred to 0, and E_r and H_r, the corresponding quantities for the reflected wave

$$\text{Then} \qquad E = E_i' - \frac{H_i' v}{c}$$

$$E_r = E_r' + \frac{H_r' v}{c}$$

$$\text{and} \quad E_i' = E_r' = H_i' = H_r'$$

Hence if E is the energy density of the incident wave referred to 0, the energy density of the reflected wave referred to 0 is $E\frac{(c+v)^2}{(c-v)^2}$

Therefore the total energy in the reflected wave referred to 0 is $EAl\,\frac{c+v}{c-v}$

This represents an energy gain of $\frac{2EAlv}{c-v}$

and hence the force is $2EA\,\frac{c+v}{c-v}$ away from the source.

R. Herman.

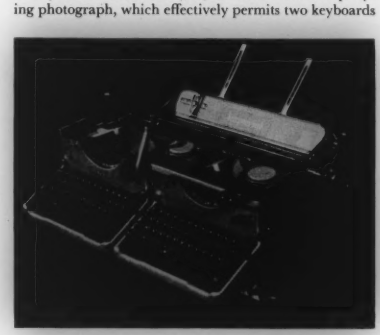
Plessey Co. Ltd., Romsey, Hants.

The scientific typewriter

Mr. Hill's letter in the November issue of the *Bulletin* about the choice of typewriter keyboard for scientific work outlines a perennial if not insoluble problem. The conventional typewriter has a very limited capacity for special symbols. It is unrealistic to speak collectively of mathematical or Greek symbols as if they could be as easily added to the machine as they can be written by hand. The typewriter manufacturers do provide special keyboards for scientific work but they include usually only a few mathematical symbols in place of some commoner literary or commercial ones. The loss of these displaced symbols is soon felt by the user. I have convinced myself that no committee would be able to specify a keyboard of conventional size that would meet Mr. Hill's needs.

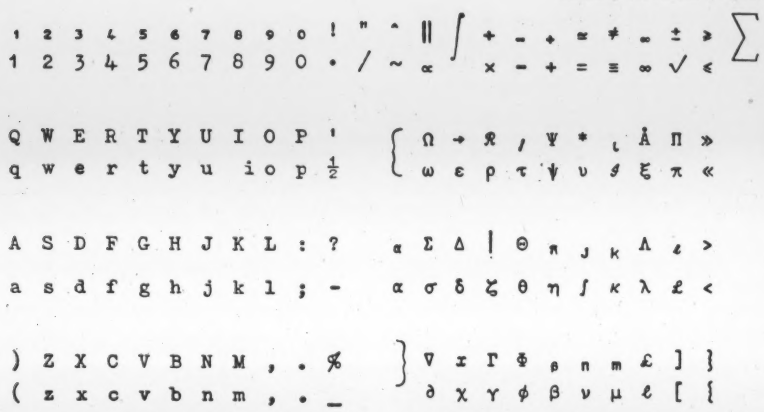
Many years ago I, too, felt the need of a special typewriter, and it was soon obvious that the capacity of about two keyboards, not of one, was needed for general coverage. The Imperial Typewriter Company have for long made machines in which the keyboard and type basket are quickly removeable as one unit from the main frame. I was able, therefore, to use two type baskets in one frame; for economy one basket was chosen to be the standard one whilst the other was specially designed to include about ninety mathematical and Greek characters. The typist had to type scientific material by using the standard basket for a few lines and then replacing it by the special basket to go over these few lines in order to insert special symbols where necessary. The particular difficulty with this approach was maintaining an exact register between the two baskets, and mechanical maintenance was frequently needed. It is perhaps only fair to the manufacturer to add that the typewriter was presumably not designed originally for such usage.

Some five or six years ago one of the National Coal Board's Research Establishments wanted a special typewriter for mathematical work and it was decided to buy the Siamesed machine, shown in the accompanying photograph, which effectively permits two keyboards



to be used together with much better register between them. In this machine the whole carriage mechanism is moved from one base frame to the other by the large black knob and arm prominently in the foreground of the photograph; the carriage mechanism can be seen locked on the nearer frame. This machine provides longer trouble-free operation than does changing type baskets, but regular maintenance is still needed to preserve perfect register between the two halves of the machine. The present retail price of such a Siamesed machine is about £170 against about £70 for the standard machine and about £40 for a spare type basket.

When two type baskets and two keyboards are available the choice of characters is wide but not wholly



free. If one standard machine is used with two type baskets the most important design criterion is the need to keep the interchange of type baskets to the minimum and some redundancy, by inclusion of the same characters on both baskets, may well be justified. With the Siamese machine this is less important as it is much quicker to change from one type basket to the other with this machine, and the machine holds its register better. When the Siamese machine was being planned we contacted a number of Government Departments, Research Associations and firms to try to obtain vicarious experience from which to evaluate the best choice of the 184 symbol positions available. A final choice was made to base one keyboard on a rationalized standard pattern, as shown on the left of the keyboard pattern. In this keyboard small numbers are in the upper case position over standard numbers and it was decided to align these small numbers so that they were subscripts on the normal line. With a half-spacing roller ratchet, as is standard equipment, one click of this ratchet moved a small number into a superscript position as for a power index, for example. The other conventional literary symbols were retained on this keyboard, but so placed that those more likely to be used came more centrally on the keyboard. Two redundancies on this keyboard were permitted, with full stops and commas, in order to keep the three lower banks the same as the conventional keyboard layout taught in touch-typing.

The other keyboard includes mathematical symbols and Greek letters. In general, Greek letters have been placed to correspond to their English counterparts on a standard keyboard but this subtlety seems to have been little appreciated by typists. Since the "hunt-and-peck" system is used on this keyboard even after the typist has had considerable experience with it, the details of lay-out seem relatively unimportant, but an effort has again been made to centralize the more frequently used characters. The final choice of the special keyboard looks haphazard because many of the Greek capital letters are identical, in type face, with the corresponding Roman capitals and are therefore omitted. The vacancies thus left have been filled partly by small letters without serifs and these are aligned, like the small numbers, to fall naturally as subscripts to other letters. Conventional vector axes and alpha, beta and pi are included in this small form as well as in normal size and alignment. The pattern of mathematical symbols follows standard recommendations wherever possible. Four particular characters take up the whole of the type bar and are used in the usual lower case shift position; these type faces are curved to fit the roller contour.

The more formal scientific reports for internal use within the National Coal Board are typed and justified on that egregious machine, the Varityper, which permits an almost unlimited choice of character.

In conclusion, therefore, I believe that a standard typewriter keyboard offers very little possibility of modification. If Mr. Hill is buying himself one I think the small numbers shown in the left hand keyboard illustrated are valuable as they permit a wide range of subscripted Roman letters to be used in place of the Greek letters which we use in scientific papers from habit. Furthermore, a number of Greek characters such as theta, phi, mu, pi and others may be simulated by two or more standard symbols. If Mr. Hill wants to produce pretty papers, some larger and more elaborate machine such as the Siamese one is needed; development of a triple machine of this type, even, is in progress. Where initial cost is not the primary consideration the Varityper and other more elaborate modifications of the conception of a typewriter would afford him still greater flexibility, but his final decision will have to be the inevitable compomise between cost and elaboration.

The author is grateful to the National Coal Board for permission to publish this letter. The views expressed are those of the author and not necessarily those of the Board.

National Coal Board, H. HERNE (*Fellow*).
18 Albert Embankment, S.E.11.

Reviews of Books

THE INDIVIDUAL AND THE UNIVERSE

By A. C. B. Lovell

London: Oxford University Press. Pp. 111. Price 10s. 6d.

This little volume reproduces almost unaltered the Reith lectures broadcast late in 1958. It is notable for outspoken statements (chapter 4) on the crisis now to be faced when public finance and policy supports scientific work whose success nevertheless depends on its freedom from political interference; Lovell allows himself the irony more often felt than expressed, in reporting that some genuine scientific advances to-day owe their existence to necessities of a past war and fears of a future war.

But the modest hundred pages also contains far the most neatly and clearly expounded account to-day of recent cosmology, both on the small scale of the solar system and the largest extragalactic scale; chapters 2, 5, 6, on this will delight physicist and general reader alike. Between the solar and extragalactic sections, chapter 3 introduces very clearly the history of radio astronomy, with the insight to be expected of this master and pioneer and with a pleasing and generous emphasis on the work of his rivals and colleagues in Leiden, Cambridge, Sydney, Russia and America.

With all this heartfelt admiration, the reviewer must also admit some disappointment that the author does not shake himself quite free from the contemporary tendency to treat the time sequence of extragalactic physics as if it were within the scope of some independent clock instead of constituting its own clock whose uniformity is therefore a speculation and whose zero is meaningless. It is doubtful whether "beginning" ought to be an allowed word in large-scale cosmology. One admits that Lovell implicitly limits "beginning" to the start of certain processes comprehensible in to-day's physical terminology, but he seems to hesitate at abandoning the naive supposition that "origin of the universe" could ever be described from within.

This carries also a serious risk that theologians, avid for the unattainable rational basis of creation myths, might see more than they ought to, in Lovell's (p. 75) apparent suggestion that there can be a passing on of responsibility "physics into metaphysics," "astronomy into theology," "science into belief," and (p. 91) that "what the primeval atom was like" could ever become a topic for philosophy or theology. One wonders whether the author may have missed his opportunity of insisting that philosophy and theology are not wise to take over the extension of arguable knowledge: for "belief" in any topic relevant to theology is not nowadays a logical enterprise, but is our honest recognition that a mystical experience which we call the presence of the divine is real but can never owe anything to, or contribute anything to, nor be opposed by the remarkable employment of spatio-temporal observation and symbolisation which we call science. One hopes that we are not now in the age of Galileo, when "belief" meant underwriting some logically arguable sequence paralleling or rivalling science—but some of Lovell's phrases might licence theologians to imagine we were.

This criticism, which is elaborated here because it requires further thought, will not deter any of us from greeting with enthusiasm these most illuminating and provocative Reith lectures yet published.

MARTIN JOHNSON.

PATTERNS OF DISCOVERY

By N. R. Hanson

London: Cambridge University Press. Pp. ix+241. Price 30s.

In this book Professor Hanson sets himself an important and somewhat novel task, namely to discuss the pattern—perhaps even the structure—of research and discovery. This is very different from relating how logical principles have developed in respect of some scientific *fait accompli*. A consequence of the author's dynamic approach is that the historical setting throughout tends to be more contemporaneous than usual, and less "after the event."

The first few chapters deal with the difficulties which are commonly encountered, both in daily life and in any observational discipline, because people not only see the same object differently, but even describe it differently. The first of these confusions has been well known to psychologists (*Gestalt*, Behaviourist, and the followers of isomorphism) for ages, and the second is bound up with Wittgenstein's language-game, particularly in its later forms. Although entertaining, it is not too obvious how far our knowledge has been advanced by these attempts to analyse everyday situations into particular forms even less amenable to practical ends.

The crux of the writer's thesis is chapter VI and appendix I, in which he is dealing with elementary particle physics. The basic proposition is that not only are elementary particles (e.g. electrons) "unpicturable," but if they could be visualized then atomic explanation would be impossible. This is a hard saying, but the evolution of thought from the time of Thales (at least) all points in that direction. To ascribe to components of any macro-assembly the properties we know such an assembly to possess is merely to shift the question back one stage, and even to make it harder to answer, since the "apparatus" needed becomes more and more fantastic. Hence it seems likely that the more earnestly basic knowledge about nature is sought, the more shall we be driven to find satisfaction in pure mathematical symbols. This does not mean, if the reviewer understands the position correctly, that we cannot use physical phenomena "mechanically" (we do so in fact in all large-scale applications), but that we cannot hope to elucidate the pattern of creation—and man's part in discerning it—by any method which uses picture-constructs to interpret the world of microphysics. Perhaps the neatest way of expressing this would be in terms of the algebra of classes, namely that the entire character of such a class is not a function of any member of that class, a result very close to the rejection of the principle of "and-summation" by the

Gestalt theorists. It is also yet another example of a "postulate of importance" in pseudo-metaphysical guise.

We are left, nevertheless, with the inquiry as to why exactly Lord Kelvin (p. 202, not p. 204 as the index suggests) could only "understand," by making mechanical models of things, and yet in spite of his laudable desire not to offend against Occam's razor, by introducing superfluous hypotheses. In the present reviewer's opinion, Professor Hanson is right in stressing the incapacity of the human mind to picture microphysical phenomena. The greatness of the Victorians was very much of a material sort ; it has taken a whole series of crises (i.e. judgments) since then to deepen our humility.

Now for appendix I. This is in effect an attempt to codify Heisenberg's most recent thinking. The trouble is largely with the formal machinery of elementary particle theory, including the apparition of certain "ghost" states without a physical interpretation. (Incidentally, something like "ghost" states are not unknown in classical physics and applied mathematics). The need is for a generalized equation for all matter, and this, in principle, Heisenberg supplies, even avoiding the snare of negative probabilities. The credit for this original idea is traced back to Anaximander. Actually, this early Greek philosopher did toy with the idea of a primordial stuff ; he was different from Anaxagosas with his "seeds," which led him inevitably to questions of particle-identity, rather than to unification of working substance.

Professor Hanson provides a set of notes at the end of his book. These are in the nature of extended references, and should prove useful.

Not everybody will agree with all that the author says, but he has elaborated an interesting theme, relevant for scientist and philosopher alike.

F. I. G. Rawlins.

RADIATION HAZARDS AND PROTECTION

By D. E. Barnes and Denis Taylor

London : George Newnes Ltd. Pp. x + 178. Price 30s.

Fifty years ago the radiation hazard was almost unknown. Injuries to patients and doctors had been reported not long after Röntgen's original discovery in 1895 but there was a persistent belief that X-rays and radium were entirely beneficial. Not until the 1920's was there any general appreciation of the damage that might be done to radiation workers through carelessness or ignorance in handling the tools of their trade. More recently the problem has taken on a new importance because every large-scale application of atomic energy generates great amounts of radiation as a by-product. In 1938 the total amount of radioactive material in Britain was under 100 curies; to-day the amounts that have to be dealt with are measured in megacuries.

Mr. Barnes, who is Superintendent of Health Physics at the Atomic Weapons Research Establishment and Dr. Taylor, lately Head of the Electronics Division at Harwell, have provided an informative account of the methods used by the United Kingdom Atomic Energy Authority, which now devotes a very large effort to the control of radiation hazards. The book has 23 short chapters, written in a pleasant straightforward style, flavoured with much sound advice based on practical experience. It starts with a description of the physical and biological properties of radiation, leading to a concise discussion of permissible dose levels. Several chapters are devoted to the measurement of radiation, including the estimation of personal exposure or internal contamination. Background monitoring, waste disposal and transport of active sources are also considered.

The scope of the book is less than its title suggests, for it covers only the hazards encountered by people working with radioactive materials. These materials are, however, used in many scientific institutions and, outside the Atomic Energy Authority, the standard of safety is often inadequate. "Barnes and Taylor" does not trip off the tongue so lightly as "Kaye and Laby" but it deserves to become just as common on library shelves and laboratory benches.

J. M. A. Lenihan.

YEAR BOOK OF THE PHYSICAL SOCIETY, 1958

London: The Physical Society. Pp. 106. Price: 12s. 6d.

Although The Physical Society, in common with many other societies to-day, is no longer able to issue its *Proceedings* to Fellows without extra payment, the *Year Book* of The Physical Society is, in many ways, an excellent substitute. In addition to such items as the Annual Report of the Council and particulars of meetings held during the year, the *Year Book* also prints all the special lectures delivered to the Society, chiefly those delivered by recipients of the Society's medals and awards. These make very interesting reading, one feature that is particularly worthy of note being that many of the lectures still retain in print the intimate character of their delivery.

The contents of the *Year Book* cover a very wide range —from spin temperatures (A. Abragam) and quantum electrodynamics (W. E. Lamb) to the physics of clouds (R. J. Murgatroyd) and the uses of physics in archæology. Accounts of the measurement of the thickness of the helium film (L. C. Jackson), the secular variation of the earth's magnetic field (E. C. Bullard) and the development of bubble chambers (D. A. Glaser) will interest specialists in these fields but to the general reader, perhaps the most interesting parts of the book are M. H. Wilson's account of Goethe's colour experiments, and E. T. Hall's account of the application of physics in archæology. Mr. Wilson's lecture deals mainly with the so-called physiological colours and is very effectively illustrated. The illustrations include part of van Gogh's "Fishing boats at Sts. Mary" as it appears in full colour when produced by illuminating a screen, in register, with blue light passed through three black-and-white positives. Mr. Hall describes the use of a wide range of physical techniques including ultrasonic cleaning (of hoards of bronze coins), proton magnetometry (for the detection of thermo-remanent magnetization of fire hearths), and magnetic and radioactive dating.

On a rather gloomier note, mention should be made of the obituary notices, including one by Mr. T. C. Keeley on Lord Cherwell.

Fellows of the Physical Society are fortunate to receive this excellent *Year Book*, which is well worth keeping, and the Society and its Editor are to be congratulated on the production.

A. E. De Barr.

TRANSISTOR ELECTRONICS

By A. W. Lo, R. O. Endres, J. Zawels, F. D. Waldhauer, Chung-Chih Cheng

London: Macmillan & Co. Ltd. Pp. xii + 521. Price 45s.

This book is written by a group of five R.C.A. engineers for advanced undergraduate or postgraduate students in electrical engineering and related fields and as a reference work for electronic engineers. Operating characteristics of transistors and their use in a wide variety of circuit applications are very fully discussed but, as usual in books of this type, the basic physics of transistors is only superficially treated. This is no doubt legitimate, but not necessarily admirable in a University text. Physical concepts are not entirely ignored, for the first 36 pages of the book are devoted to them, and there is a further section of about 37 pages on the physical interpretation of transistor parameters which is of great interest. The circuit applications of transistors are discussed exhaustively and there are useful sets of problems at the ends of chapters.

There are chapters on general characteristics, basic amplifier configurations, d.c. bias circuits, l.f., power and h.f. amplifiers, h.f. operation, oscillators, pulse circuits and modulation, in addition to the above mentioned chapters on the basic mechanisms of transistors.

J. D. Craggs.

SOME ASPECTS OF ANALYSIS AND PROBABILITY

Contributors: Irving Kaplansky, Marshall Hall, Jr., Edwin Hewitt, Robert Fortet

London: Chapman and Hall Ltd. Pp. xi + 243. Price 72s.

This is one of five Surveys in Applied Mathematics published as a joint project of the United States Office of Naval Research and Applied Mathematics Reviews. This project arose out of the increasing realization that there existed major areas of modern mathematics and theoretical mechanics the scope and trend of which were incompletely conveyed by the Western literature alone, quite apart from missing particular recent results. It is the purpose of these surveys to remedy this defect by giving particular attention to significant articles which have appeared in inaccessible journals or unfamiliar languages.

To the present book each of the four authors has contributed a section. These are, in order, Functional Analysis, A Survey of Combinatorial Analysis, A Survey of Abstract Harmonic Analysis, and Recent Advances in Probability Theory.

These apparently diverse topics are held together by their extensive use of abstract function spaces reflecting the process of abstraction which is characteristic of much of modern mathematics and theoretical mechanics. Perhaps this is to be regarded as the opposite pole to the extensive work in numerical analysis associated with the development of digital and analogue computers.

The first section starting by reviewing the problems posed by Banach, goes on to discuss locally convex spaces and their operators, Banach algebras and group representations. There is a passing reference to Schwartz' distributions but attention tends to concentrate on the work of Silov, Gelfand and Naimark on commutative Banach algebras, much of which is only available in Russian.

The second section offers perhaps more interest to the non-specialist reader, partly because the author starts at a more elementary level, no doubt because it is a new and not very clearly defined subject. The author defines it as the problem of arranging objects according to specified rules and finding out in how many ways this can be done.

Partly also because some progress can be made by quite elementary methods and because some of the problems are less abstract in character such as the assignment problem and the travelling salesman problem which have achieved some notoriety, or the directly practical problems of scheduling of tankers and construction of production scheduling arrangements for which the General Electric Company has been using high speed digital computers.

In the years 1925-1935 the theory of topological spaces had reached a point at which it could be seen that many important propositions on the theory of Fourier series and integrals were translatable into propositions about functions, measures, etc., defined on topological groups. Here we have the beginning of abstract harmonic analysis, reviewed in the third section.

The final section starts with the Kolmogorov axioms, and discusses sums of random variables, general random elements, the Theorems of Kolmogorov and Smirnov and finishes on tests and estimations related to stochastic processes.

All four sections contain extensive bibliographies and the book will be invaluable to mathematicians who are interested in these topics.

A. W. Gillies

CONCEPTS OF CLASSICAL OPTICS

By John Strong

San Francisco: W. H. Freeman & Co. Pp. xxi + 692. Price 80s.

I think the features which make a good textbook are the following:

(*a*) An account of the substance of the subject claimed in the title written plainly and at a modest level of difficulty for the student.

(*b*) Absence of material which is not of importance to the student.

(*c*) Conveyance of a feeling that the subject is still alive and worth knowing about otherwise than as a mere "intellectual discipline."

(*d*) In physics textbooks, a preference for physics rather than mathematics.

(*e*) Enough originality to make the book worth reading

by teachers in the hope that they might find something useful in their teaching.

Judged by these criteria *Concepts of Classical Optics* comes out very well. The subject indicated by the title occupies rather more than half the total number of pages. Then follows a series of 16 appendices, taking up about a third of the book, on topics of current interest, such as apodization, radiation detectors, microwave experiments and wave-theory of microscopic image formation, each written by an expert. "A Mathematical Review" occupies 41 pages and succeeds in an interesting way in giving an account of the mathematics necessary for understanding the book. Finally there is a collection of problems for the student.

Roughly speaking the fundamental theory of optics may be divided into two parts, namely, the mathematical device originated by Huygens for tracing the progress of waves, and the discussion of the scattering of electromagnetic waves by electrons. When the scattering is coherent we can apply Huygens' device and, at least in principle, arrive at physical theories of refraction and reflection. Together with the supplementary ideas which are necessary for understanding interference, diffraction and the mode of action of optical devices these are the author's *Concepts of Classical Optics*. The student who reads the book should acquire a physicist's understanding of optics. He should also acquire useful knowledge of optical instruments, much of which is embodied in the appendices. These, as the author says, "are intended to give the student the flavour of current activities and interests."

The book is attractively produced. Readers who know the author's previous book, *Modern Physical Laboratory Practice*, will recognize the same "free-hand sketch" style of the illustrations to this one. In many cases they are attractive and effective, but occasionally one would prefer the more usual kind of diagram.

E. W. H. Selwyn.

RADIOISOTOPE LABORATORY TECHNIQUES

By R. A. Faires and B. H. Parks

London: George Newnes Ltd. Pp. xii + 244. Price 25s.

This book is intended by its authors to provide an introduction for the non-specialist scientist or technologist to the potentialities of the many new techniques made possible by the use of radioisotopes. The authors, who have for many years been connected with the Isotope School at Harwell, are particularly well qualified for their task.

The book, which has twenty chapters, six appendices and an adequate index, is divided broadly into four sections of roughly equal size. The first deals with the basic principles of nuclear physics and with protection; the second with laboratory design and procedures; the third with the statistics of counting and with methods of detection and measurement; and the fourth with a review of the techniques themselves and of the various applications, as well as with the feasibility of using isotopes in a particular system.

The treatment is practical rather than theoretical and the book is intended to act as a manual for the man in the laboratory rather than as a textbook. Enough of the fundamental material is, however, included to enable a reader, previously unacquainted with the field, to grasp the essentials and to proceed cautiously but safely with experimental work. In fact, so much detailed information is given in respect of equipment, which is almost without exception of British manufacture, that the authors seem to have limited the market for the book to those for whom such equipment is a matter of choice.

In view of the general excellence and usefulness of the book, it is unfortunate that the text is marred by a number of errors, most of which are trivial and which it is hoped will be corrected in the next impression. An example of the errors is the statement on p. 40 that the maximum permissible level of dose-rate (to the whole body) is 0·075 rad/hour when, from the context, 0·0075 rad/hour is intended. This error, which occurs in the chapter on health physics is particularly unfortunate, but one can find no fault with the statement at the end of this chapter which stresses that the various permissible doses and radioisotope concentrations are *maximum* levels and continues "They may change, but the need to keep levels to a minimum will not."

The book can be recommended to all those who are thinking of entering the field for the first time and it may well be that many who are already in the field will find something new in it and hence profit from a study of its pages.

E. E. Smith.

News of Members

FELLOWS

H. C. von Bertele has been appointed to the new chair of Industrial Electronics at Vienna Technical University.

Sir John Cockcroft has been appointed the first Master of Churchill College, Cambridge.

J. V. Dunworth is now Assistant Director, Atomic Energy Research Establishment, Harwell.

R. F. Farr has been appointed Chief Physicist to the United Birmingham Hospitals, at the Queen Elizabeth Hospital, Birmingham.

Roy C. Faust has taken an appointment with Imperial Chemical Industries Ltd., Alkali Division, Winnington, Northwich, Cheshire.

R. G. Friend has been appointed Director of Electronics Research and Development (Telecommunications) in the Ministry of Supply.

Kathleen A. Gough has been elected an M.I.E.E. She is only the second woman to be elected to this grade; the previous election of a woman was in 1899.

Sir Leslie Martin, Professor of Physics in the University of Melbourne, has been appointed the first Chairman of the Australian University Grants Committee.

Professor N. F. Mott has been elected Master of Gonville and Caius College, Cambridge.

Professor H. W. B. Skinner has been elected a member of the Council of The Royal Society.

G. R. Richards is a Senior Principal Scientific Officer with U.K. Ministry of Supply (A), Salisbury, S. Australia.

C. A. Taylor has been promoted Reader in Physics in the University of Manchester.

Hugh C. Tett has been appointed Chairman of the Board of Directors of the Esso Petroleum Co. Ltd.

C. S. Venkateswaran is now Director of Collegiate Education, Eastern Nada, Irinjalakuda, Trichur St., Kerala State, India.

A. G. Ward has been appointed Professor of Leather Industries in the University of Leeds.

N. Winogradoff has joined the Semiconductor Department of the International Business Machines Research Center at Poughkeepsie, New York, U.S.A.

R. L. Woolley is now with the Central Electricity Generating Board at its London headquarters.

ASSOCIATES

Margaret H. Alston has taken up an appointment with the Radiation Laboratory, University of California, Berkeley, U.S.A.

D. E. T. F. Ashby is now engaged in thermonuclear research at Harwell.

C. J. Ball is now a Lecturer in the Physics Department at Makerere College, Kampala, Uganda.

R. R. Birss is now at the Imperial College of Science and Technology, London.

R. F. Blackwell is in the Commissioning Department of the A.E.I.—John Thompson Nuclear Energy Co. Ltd., Knutsford, Cheshire.

C. E. Brindley is now employed as a Physicist by the U.K. Atomic Energy Authority at their Capenhurst factory, Nr. Chester.

H. Burns has been appointed Experimental Engineer at British Nylon Spinners Ltd., Pontypool.

Ian Butterworth has taken up an appointment as Lecturer at the Imperial College of Science and Technology, London.

R. W. Cahn has been appointed Reader in Physical Metallurgy at the University of Birmingham.

Kenneth G. Carr is Deputy Headmaster at the Grammar School, Spalding.

J. Kenneth Claxton is now Senior Lecturer in Applied Physics at Twickenham Technical College.

Miss M. W. Clutterbuck is now at Watford Technical College, Hempstead Road, Watford, Herts.

S. W. R. Cox is a Principal Scientific Officer at the National Institute of Agricultural Engineering, Silso, Beds.

D. A. Davies is now employed at Siemens Edison Swan Research Laboratories, Harlow, Essex.

F. R. Dean is a Lecturer in Physics at Harris College, Preston.

John McK. Ellison is now at the Medical Research Council Group for Research on Atmospheric Pollution, Dunn Laboratories, St. Bartholomew's Hospital, London, E.C.1.

D. W. Evans has joined the Atomic Energy Research Establishment, Harwell.

Raphael Fletcher has taken up an appointment as Research Assistant in the Department of Physics, Manchester College of Science and Technology.

E. V. Gilby is now a Physicist at the Industrial Group Headquarters of the U.K. Atomic Energy Authority at Risley, Lancs.

H. W. Gosling is in the Department of Engineering, University College of Swansea.

I. H. Hall has joined the staff of the British Rayon Research Association, Heald Green Laboratories, Wythenshawe, Manchester.

S. M. Hamberger has taken up an appointment as Senior Scientific Officer in the Controlled Thermonuclear Research Division at Harwell.

J. K. Hargreaves is now working as a Technical Officer in the Research Department of I.C.I. Ltd. (General Chemicals Division), Widnes.

J. H. Harris is a Lecturer in Physics at Sunderland Technical College.

E. D. Hart is now with Industrial Exhibitions Limited, London.

M. H. J. Hawkins is on the staff of Bexley Grammar School, Welling, Kent.

G. Jackson has been appointed a Health Physicist at the Hunterston Nuclear Power Generating Station.

G. E. Linton has been appointed to a Lectureship in Physics at Gloucester Technical College.

T. B. Massalski is now Senior Fellow and Head of the Metal Physics Section of the Mellon Institute, Pittsburgh, Pa., U.S.A.

P. W. R. Morpurgo is now in the Department of Human Nutrition at the London School of Hygiene and Tropical Medicine.

H. S. Payman has been appointed Managing Director of Besson & Robinson Ltd., London.

J. Potts has been appointed head of a section of the Research Department of Short Bros. and Harland Ltd., Belfast.

N. Ramanathan has joined the National Research Council of Canada as a postdoctorate fellow.

Bernard Roberts is now with the Textile Development Department of British Nylon Spinners Ltd., Pontypool Mon.

G. S. Rose has been appointed an Assistant Research Officer in the Division of Applied Physics of the National Research Council, Ottawa, Canada.

D. R. Scott is Head of the National College for Heating, Ventilating, Refrigeration and Fan Engineering, Borough Polytechnic, London.

E. R. Drake Seager has been appointed Principal Scientific Officer at the Atomic Weapons Research Establishment, Aldermaston.

R. S. Sharpe is now with the Atomic Energy Authority at Harwell.

J. J. Singh has taken up an appointment as a Research Associate in Physics at the University of Kansas, U.S.A.

A. H. Slater is a Development Physicist in the Technical Development Department atthe Ravenhead Works of Pilkington Bros. Ltd., St. Helens.

R. N. Stone is now in the Technical Research Department of A. Reyrolle & Co. Ltd., Hebburn, Co. Durham.

R. W. Taylor has taken an appointment as Senior Development Engineer with Associated Transisters Ltd., South Ruislip, Middlesex.

P. Tothill is a Principal-grade Physicist with the Medical Physics Unit, Edinburgh.

D. Weatherley is Senior Physics Master at Ryhope Grammar School, Nr. Sunderland.

R. A. Wilson is now at The High School, Klang, Selangor.

GRADUATES

W. F. Archenhold has been appointed Head of Physics Department at Edward Shelley High School, Walsall.

P. Benjamin is now a Capacitor Engineer with the Standard Telephones and Cables Ltd., North Woolwich.

G. R. Blackwell is a Physicist at the Hawker Siddeley Nuclear Power Co. Ltd., Langley, Bucks.

Peter R. Blakey has been appointed to a Fellowship at The Bradford Institute of Technology.

J. F. D. Butterworth is now a Research Physicist with British Cellophane Ltd., Bridgwater, Somerset.

L. Davidson is now employed by the United Steel Companies Ltd., at their Research and Development Laboratories at Rotherham, Yorks.

A. Earl has been appointed an Assistant Lecturer in Physics at Derby and District College of Technology.

A. Fernández is now at the Instituto de Física, México.

N. T. Gallagher is now employed as a technical sales representative with Leybold Vacuum Sales, London, W.1.

Richard J. Griffiths is now with the Atomic Energy Research Establishment, Harwell.

H. A. Hajjarizadeh has been transferred to the Process Engineering Department, The Refinery, Abadan, Iran.

Dan Haneman is now in the Physics Department of Brown University, Providence, Rhode Island, U.S.A.

D. C. E. Hanney has taken an appointment as a senior engineer with Standard Telephones & Cables, Ltd., New Southgate Works.

R. M. Hudson is now employed as an Assistant in the Natural Philosophy Department of the University of Edinburgh.

M. Kirton is with the Australian National Antarctic Research Expedition until March, 1960.

R. B. D. Knight is now with Solartron Research and Development Ltd.

J. Kosky has been appointed Research Officer with the British Paper and Board Industry Research Association, Kenley, Surrey.

Desmond Lamb is now with the Weapons Research Establishment, Salisbury, South Australia.

L. A. Law has joined the Canadian Westinghouse Co. Ltd., Hamilton, Ontario.

Mrs. M. E. Law has accepted a post as a Research Fellow at McMaster University, Hamilton, Ontario.

G. S. Lawson is now an Experimental Officer in the Mechanical Engineering Research Establishment, East Kilbride.

A. Levy has taken up an appointment with the Broken Hill Pty. Central Research Labs., Newcastle, N.S.W., Australia.

S. T. Loney is now with the Research Department of Smith's Aircraft Instruments, Bishops Cleeve, Cheltenham.

P. Mawdsley has taken the position of Supervising Computer Engineer with Ferranti Ltd., Manchester.

W. Stewart Nicol is now employed in the Lincoln Laboratory of the Massachusetts Institute of Technology.

T. F. J. Quinn has been promoted to Research Physicist at Associated Electrical Industries Ltd.

K. M. Rose is now Principal Scientific Officer in the Research and Development Branch of the Springfield Works of the U.K. Atomic Energy Authority, Industrial Group.

D. J. Sansom is in the Magnetic Materials Section of Siemens Edison Swan Research Laboratory, Harlow, Essex.

J. F. Sayers is now an Experimental Officer in the Metallurgy Division of the Atomic Energy Research Establishment at Harwell.

M. T. Sprackling is physics master at King Edward VI Grammar School, Stourbridge, Worcester.

Reginald Sykes is now employed as a Physicist in the Physics Research Department of Turner Bros. Asbestos Co. Ltd., Rochdale, Lancs.

Derek L. Thomas has taken up a post as an electronic engineer with Ferranti Ltd., Edinburgh, in their Radar & Navigational Aid Department.

H. C. Watson is now in the Biochemistry Department, College of Physicians and Surgeons, Columbia University, New York, U.S.A.

J. L. Wright is now a Senior Engineer in the Telecommunications Group of the Plessey Company, Ilford.

Group News

Non-Destructive Testing Group: a Vote of Confidence

The Non-Destructive Testing Group, probably in common with other of the Institute's Groups, has quite naturally built up a hard core of members from amongst those who personally have had the task of applying physical methods of inspection and analysis to the various needs of industry. It is of course abundantly realized that when newly developed techniques are being introduced, it is the invaluable practical experience of such members that points the way to further technical refinement and immediate development. However, there probably comes a time when the methods for which the specialist Groups were formed become suffi-

ciently established that continued discussion and contribution tends to become repetitive and associated more with the technology of the "fringe" subjects to which they become integrally attached.

An upsurge of interest in non-destructive testing began to permeate industry some four or five years ago as more and more exacting demands were made upon materials in some of the newer and expanding fields where service reliability is of paramount importance. In its wake, both national and international bodies were established, and in this country one independent body in particular, the Non-Destructive Testing Society of Great Britain, was formed to look after the technological aspects of the subject, which had begun to infiltrate into the Group's activities and showed every likelihood of becoming a major part of the proceedings.

The Group at this time began to pass through a period of functional re-orientation, and is now emerging, and becoming accepted, as the authoritative body responsible for examining the physical background of the present inspectional techniques and of developing and encouraging new methods. Its future programs are being planned accordingly.

This line of approach is bound to have dissociated part of the Group's activity from the established techniques. Instead, more effort is being channelled into a closer and detailed study both of basic structural changes and of the future inspectional needs of industry as it strives for more automatic production, and more consistent products to operate under increasingly stringent service conditions.

In adopting this newer approach to foster further advances in the subject, the Group Committee realized that many members may not fully have appreciated the needs prompting so apparent a diversion from the policy that had evolved since the Group's formation. Also, in order to implement this new approach, the Committee felt that the Group must concern itself with a wider field of physics activity. Indeed to function properly it should interest and attract those physicists working in the basic fields of materials research and invite their membership and participation.

With these thoughts in mind it was decided to open the new session on October 17 with a debate and general discussion on the motion "That this House considers that the Non-Destructive Testing Group serves no useful purpose."

The task of proposing and seconding the motion was left to two Committee members, Mr. W. G. Cook and Mr. B. D. Burns who were well acquainted with the arguments of those members who think that the Group in its new guise is failing to incline a sufficiently attentive ear to the immediate needs of industry. Some of the criticisms levelled at the present policy were:—

1. Too ambitious a line is being adopted and too wide and nebulous field is being examined on too limited an organizational potential.
2. The present industrial inspection needs are extensive and complex enough without attempting to examine future requirements.
3. Too few meetings are held out of London, and not enough are organized co-operatively with other bodies.
4. A "physicist versus technologist" complex is going to pervade the Group with the result that it will in time become completely divorced from the industrial atmosphere, and so lose industrial support and backing.

This type of criticism is perhaps inevitable if the Group's activities are viewed out of the context of the present national organization for Non-Destructive Testing which, when fully operative, should both cater for, and integrate, the subject in all its many aspects.

The prospects of diminishing progress envisaged by some speakers reflected in fact that the present mechanism of sub-divisional effort is either inadequately understood or, in its infancy, is insufficiently organized or advertised.

Some, feeling that the present system of diversified interest was inherently inadequate, expressed an urgent need for a unified national society for Non-Destructive Testing, with sufficient financial backing, that would be capable of dealing, through a centralized organization, with all aspects of the subject. As a long-term solution this may well be the most appropriate arrangement.

To oppose the motion, and so support the Group's present policy, Dr. E. G. Stanford and Mr. N. Clarke were chosen as principal speakers. No one has championed the present sway in Group policy more than Dr. Stanford and to him must be given the credit for much of the foresight and planning that has led to the present situation. Mr. Clarke, on the other hand, as a permanent officer of the Institute was able to present the Institute's interpretation of the true purpose of Group organization, which is to formulate and disseminate the physics of applied techniques.

They replied to the criticisms by stressing the futility of not striving constantly outwards—to new methods, new ideas, new understandings, new approaches and new horizons. There was a need at the same time for developing educational facilities so that all involved in the subject—the physicist, the metallurgist, the industrialist and the instrument maker become mutually informed of requirements and progress, and this it was felt, was being tackled realistically.

After a lively discussion, it should perhaps be noted, for the record, that no one voted in favour of the motion. However, the chairman thoughtfully let the matter rest there rather than divide the audience by calling for abstainers.

Having explained its policy in this way to its members, the Group is now planning a more ambitious discussion meeting on March 13 under the chairmanship of Dr. Sykes. Its theme is to be "What Industry requires from non-destructive testing."

A number of invited speakers will present to the meeting their personal views on future inspectional requirements as they affect various industries. These will then be critically examined in the light of the adequacies, or otherwise, of the present techniques and foreseeable developments. Proposals will then probably be forthcoming as to how more concentrated effort and more controlled development can be brought to bear on the problems, and along which lines the maximum effort should be channelled.

Such a discussion is distinctly relevant to the policy now being adopted by the Group and may well result in a significant forward step in co-operative endeavour.

R. S. SHARPE.

Non-Destructive Testing Group Dinner

The second of the Group's annual dinner meetings was held in London at the Horseshoe Hotel in Tottenham Court Road on January 16. About 50 members and their ladies attended.

The talk following the dinner was given by Mr. S. Rees Jones, F.Inst.P., of the Courtauld Institute of Art on "Testing of Works of Art and the Detection of Forgeries." He explained how an intimate knowledge of the evolution of the painter's art, and the techniques and mannerisms of individual Masters, was essential in order to explain fully the finer points of indentification. A series of illustrations served to emphasize his remarks and to show how much additional information could be obtained by a judicious use of the available "non-destructive" tests that could now be called in to aid the art historian.

R. S. SHARPE.

Forthcoming Institute Meetings

Date	*Place*	*Speaker*	*Title*	*Time*	*Branch or Group*
Mar. 16	**Manchester** The University	Dr. A. G. Gaydon, A.Inst.P., F.R.S. (*Imperial College of Science and Technology, London*)	**Recent developments in techniques for the spectroscopy of flames**	6.45 p.m.	Manchester and District
Mar. 17	**London** The Institute's House	Dr. E. G. S. Paige (*Royal Radar Establishment, Malvern*)	Group Annual General Meeting **Electrical effects of dislocations in germanium**	5.30 p.m. 5.40 p.m.	Electronics
Mar. 18	**Birmingham** The University	Prof. J. D. Bernal, F.Inst.P., F.R.S. (*Birkbeck College, London*)	**The physical basis of life**	6.30 p.m.	Midland
Mar. 19	**London** The Institute's House	Prof. O. R. Frisch, F.R.S. (*Cavendish Laboratory, University of Cambridge*)	**The new particles of physics**	6.00 p.m.	London and Home Counties
Mar. 19	**Newcastle-upon-Tyne** King's College	Mr. I. M. Ross (*Services Electronics Research Laboratory, Harlow*)	**Masers and reactance amplifiers**	6.15 p.m.	North Eastern
Mar. 31 to Apr. 4	**Delft** Netherlands Technische Hogeschool		**A conference in collaboration with the Instituut T.N.O.**		Stress Analysis
	Details and application forms can be obtained from the Secretary of the Institute.				
April 2 and 3	**London** The Institute's House		Easter Conference on : **Teaching of science to children up to the age of fifteen**		Education
	Registration is necessary and attendance is limited. Further details will be circulated to Education Group members and may be obtained by others from the Secretary of the Institute.				
April 6	**Bristol** The University	Prof. A. C. B. Lovell, F.Inst.P., F.R.S. (*University of Manchester*)	**The program of the radio telescope at Jodrell Bank**	7.00 p.m.	South Western
April 7	**Southampton** The University	(Jointly with the Physical Society Acoustics Group)	A Symposium on : **New techniques in the analysis of noise and vibration**	10.15 a.m.	London and Home Counties
April 9	**Liverpool** The University	Dr. N. Kurti, F.Inst.P., F.R.S. (*University of Oxford*)	**Nuclei at very low temperatures**	7.00 p.m.	Liverpool and North Wales
April 10	**Sheffield** Conference Room B.I.S.R.A.	Prof. G. A. Homès (*Polytechnic Faculty, Mons, and University of Brussels*)	**The physical background of existing techniques and the relationships between physical and structural properties**	6.30 p.m.	Non-Destructive Testing
April 14	**Isleworth Middlesex**	Mr. I. Evans, F.Inst.P. (*Central Research Establishment, Isleworth*)	Visit to the Mining Research Establishment, followed by a lecture on **The fracture of brittle materials**	2.15 p.m. 5.45 p.m.	London and Home Counties
	Registration is necessary and attendance is limited. Further details will be circulated to London and Home Counties Branch members and may be obtained by others from the Secretary of the Institute.				
April 17	**Birmingham** University Union	Further details will be sent to Midland Branch members.	**Branch Annual Dinner**	7.00 p.m.	Midland
April 17	**London** Royal Society of Tropical Medicine		Symposium on : **Current developments in the production of high vacua** (Details from the Institute)	10.00 a.m.	

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The Institute's Journals

PRINCIPAL CONTENTS OF THE MARCH, 1959, ISSUES

Journal of Scientific Instruments : Volume 36, No. 3

Theory and design of getter-ion pumps. By L. Holland, F.Inst.P.

The theory and design are discussed of getter-ion pumps, in which gas is exhausted by sorption in metal layers in the presence of an ionizing electron stream. The development of getter-ion pumps is traced from the early observations of gas absorption in glow-discharge tubes and sputtered and evaporated deposits. The effects of activating the residual gas by ionization are discussed and it is shown that only a limited number of gas-metal pairs are known where this enhances the sorption rate. Generally, the speeds of oxidizing reactions are not influenced by activation. Inert gases, which cannot be directly sorbed by the getter, may be ionized and their positive ions transported to a target, where they may be embedded or covered by metal films. The sorption and deposition properties of a range of getter materials are surveyed, and it is shown that titanium, barium and calcium may be used in evaporation type pumps. Methods of continuously replenishing the gettering surface by vacuum evaporation and cathodic sputtering in a range of pumps are described. The use of getter-ion pumps with mechanical and diffusion pumps for pre-exhaustion and obtaining very low pressures are discussed, and factors limiting the ultimate pressure considered. The performance of getter-ion pumps is compared with that of diffusion pumps and it is shown that getter-ion pumps may be used where the gas throughput is small, e.g., attaining *ultra high vacuum* in degassed envelopes and maintaining the vacua in electronic apparatus of limited volume.

High-temperature X-ray diffractometer. By J. Spreadborough and J. W. Christian, A.Inst.P.

A vacuum furnace designed for use with a commercial diffractometer is described. It uses a high-current heater made from tantalum foil, and allows the full Bragg spectrum to be recorded whilst satisfying the Bragg-Brentano focusing condition. The specimen temperature is uniform and can be measured accurately; the furnace has been operated successfully at temperatures up to 1100°C, and it is believed that considerably higher temperatures can be achieved.

Acoustic gas analyser. By L. Molyneux.

An instrument capable of measuring the concentration of halothane (bromochlorotrifluoroethane) or chloroform vapour over a range of 0–4% by volume, with an accuracy of ±½%, is described. The gas containing the vapour is a mixture of air and oxygen, and the instrument compares the time taken by a pulse of sound to travel through the original gas sample with the time taken for another pulse to travel through the same sample after the removal of the vapour by activated charcoal. Transistors are used throughout, and the instrument is small and self-contained.

Electrolytic hygrometers for measurements in gases down to a few parts per million. By J. K. Crawshaw, A.Inst.P., and F. G. Davidson.

Another method for making electrolytic hygrometers is described, together with detailed measurements of their characteristics. The hygrometer is well-fitted for continuously recording the water content of suitable gases in ranges down to a few parts per million. An instrument is described for making rapid specification tests on cylinders of the refrigerant dichlorodifluoro-methane.

Photographic method for pulse amplitude analysis. By P. J. Kennedy and P. J. Dean, Grad.Inst.P.

The problem of determining the magnitude distribution of fast rising voltage pulses is of frequent occurrence. A method is described by which such pulse height spectra may be obtained by photographing a cathode-ray oscilloscope display, in which the only tops of the pulses appear on the screen. The time base is switched off and the exposure is adjusted to permit the film to integrate the blackening due to a large number of individual pulse tops. The density variation on the film may be shown to be closely related to the pulse height spectrum and may be obtained from a densitometer measurement. Full details are given of the circuits used to obtain such a display, together with a summary of the applications to which the technique is particularly suited.

Electrometer for measuring the variation with concentration of the low frequency dielectric constant of aqueous ionic solutions. By V. I. Little, A.Inst.P.

A new electrometer is described which enables measurements to be made of the dielectric constants of aqueous ionic liquids, in terms of that of pure water, up to concentrations of about 10^{-2} N. The method depends on the observation of a null point when two couples acting upon an electrode suspended in the liquid are equal. One couple depends on electric forces and varies directly with the dielectric constant, whilst the other couple is magnetic and may be made proportional to the value of an external resistance. The probable error in the final results is of the order 0·5%.

Large scale laboratory freeze-drying apparatus. By P. M. Grant and R. B. Ward.

An apparatus is described which will freeze-dry a total of 6 l. of solution distribution in twenty flasks each twenty-four hours, with a minimum of attention and at a low operating cost.

Cryostat for measuring the electrical properties of high resistance semiconductors at low temperatures. By W. H. Mitchell and E. H. Putley, A.Inst.P.

This apparatus enables measurements to be made on semiconductors of resistance up to $10^{11}\Omega$ at temperatures down to 2°K. A vibrating reed electrometer is used for the measurements. The characteristics of suitable instruments are described and the performance of the apparatus is illustrated by showing some results obtained for a specimen of gold doped silicon. The maximum specimen resistance which can be used is determined by the self-capacity of the apparatus. The noise level in the electrometers sets a lower limit for the measurement of Hall mobility at about 10 $cm^2\ V^{-1}\ S^{-1}$.

Method of determining the dynamic constants of liquid-containing membrane manometer systems. By R. J. Linden.

An instrument with which to produce sine and square wave changes in pressure in the hydraulic systems of membrane manometers is described. It consists of a cylindrical chamber to one end of which a metal membrane is secured; fixed to the membrane is a barium titanate strain gauge which, when energized by alternating voltages, causes alternating pressure changes in the chamber. The theory on which the construction of this instrument is based is substantiated by experiment. The use of the instrument in the examination of a damped capacitance manometer system is described.

British Journal of Applied Physics : Volume 10, No. 3

CONFERENCE REPORTS

Summarized proceedings of a colloquium on X-ray fluorescence analysis—London, February and April, 1958.

X-ray fluorescence as an analytical technique was the subject of a joint meeting of the Applied Spectroscopy and X-ray Analysis Groups of The Institute of Physics held in London on February 21, 1958 (and repeated on April 1).

Summarized proceedings of a conference on X-ray analysis—Manchester, April, 1958.

The annual Spring Conference of the X-ray Analysis Group of The Institute of Physics was held at the Manchester College of Science and Technology on April 18 and 19, 1958. The papers and discussions ranged over a wide field including structural studies, apparatus and techniques of analysis.

ORIGINAL CONTRIBUTIONS

Evaluation of approximate methods of applying binary scale factors in pulse-counting systems of measurement. By W. T. Bane and M. P. Atkinson.

An approximate system of applying binary scaling factors in pulse-counting systems of measurement is described and the errors involved are evaluated. This system forms the basis of a series of developed systems whose errors are also discussed. The effects producing the errors are not arithmetic, and complete proofs are lacking for some of the expressions, which, however, have been checked over the first part of their range.

Some experimental results on the impact sensitivity of mercury fulminate. By M. P. Murgal and A. K. Ray.

An account is given of some experimental results, namely, 50% explosion efficiency heights, times of impact and pressures on the impact sensitivity of mercury fulminate. These pressure estimations, together with similar measurements by other workers, have been compared to the theoretical calculations by one of the authors in an earlier publication. There is a general agreement between the experimental and theoretical pressures.

Alternative method of correlating forced convection heat transfer data. By P. H. Price.

The use of a heat transfer coefficient as a general parameter of forced convection heat transfer is shown to be unsatisfactory, and an alternative correlation making explicit allowance for pipe wall temperature variations is derived. In the derivation it is assumed that radial heat transfer in the fluid may be represented as due to an effective conductivity and that longitudinal heat transfer is by fluid transport only. The effective conductivity and the fluid forward velocity may be arbitrary functions of radial distance.

The correlation shows that the difference between wall and mean fluid temperature at any distance along the tube is composed of an entry condition term which diminishes with distance along the pipe, plus a series of terms due to temperature variations along the pipe wall. The values of the coefficients of these terms have been calculated for two ideal cases, and experimental values are being sought. Some implications of the correlation are discussed.

Method for size-distribution determinations of non-volatile droplets by electron microscopy. By W. J. Harris.

The droplets are sampled on a thin wet film of gelatin solution and washed away when the gelatin has set. Replicas are taken of the impressions left by the spread-out droplets. The replica diameters are converted to droplet diameters by the application of a spread factor which although determined for larger droplets by light microscopy may be reliably applied to droplets in the electron microscope size range.

New method for increasing the X-ray reflection power of lithium fluoride crystals. By E. F. Priestley.

Bending and quenching methods for increasing the dislocation density and hence the X-ray reflection power of lithium fluoride have been found ineffective when applied to certain crystals of British origin. An ultrasonic treatment is described which, with abrasion of the surface, gave a gain in reflection intensity of up to 3½ times at 2·0Å and 7 times at 0·6Å.

Ferromagnetic after-effect in mumetal. By L. Castelliz and W. W. H. Clarke, A.Inst.P.

Study of the magnetic performance of Ludlam mumetal sheets incorporated in extremely precise deflexion yokes has revealed a magnetic after-effect having a magnitude and decay time which appear to place it in a different category from reported phenomena of this general nature. This paper describes the phenomonon, which has been measured, and includes a description of the mechanism which is believed to be responsible for it. The effect is partially submerged by the well-known eddy current delay, measurements of which have also been made. The dependence of both after-effects on various parameters such as annealing treatment, thickness and spacing of the mumetal sheets and temperature, has been revealed qualitatively by the experiments, and it is the difference in their behaviour under these parametric variations which has enabled them to be properly distinguished.

Production of X-rays during a low-pressure gas discharge. By R. F. Thumwood.

The impulse breakdown of several gases at pressures of a few microns of mercury has been investigated. During the first stage of the discharge, a microsecond burst of X-rays is emitted from a spot of about 8 mm diameter on the anode electrode. If the initial voltage applied to the tube is 600 kV, a single X-ray pulse will produce a legible radiograph through about 5 cm of steel.

The mechanism of the discharge has been studied and high-speed photographs of the visible discharge show a luminous column moving from cathode to anode. Steenback's theory has been extended to show qualitatively the probable sequence of events during the discharge.

CENTRAL ELECTRICITY GENERATING BOARD

Applications are invited for appointments as

SENIOR and JUNIOR PHYSICISTS

in the Research and Development Department.

The successful candidates will join a team studying graphite in the Board's nuclear reactors, and there may also be opportunities for fundamental research. The posts will be tenable, initially, at London Headquarters but will be transferred later to the Board's nuclear laboratories at Berkeley, Glos.

Candidates for the senior posts should have a degree in physics, and postgraduate experience though not essential would be an advantage. For the more junior posts, a qualification in physics such as H.N.C., Inter. B.Sc., or equivalent, is desirable. If necessary, suitable training will be arranged.

Salaries on scales within the overall ranges £1,195–£1,775 p.a. and £570–£1,300 p.a. according to duties and responsibilities, plus London Allowance when appropriate.

Application forms obtainable from:

The Personnel Officer, 24/30 Holborn, London, E.C.1,

should be completed and returned as soon as possible.

Please mark envelopes "Confidential Ref. PB/58."

THE MANCHESTER COLLEGE OF SCIENCE AND TECHNOLOGY

(Faculty of Technology in the University of Manchester)

APPOINTMENT OF DEMONSTRATOR IN PHYSICS

The Governing Body invites applications for a Demonstratorship in physics. The Demonstrator will be required to assist the teaching staff in practical classes and in tutorials. In addition he will be allowed ample time for research and will be expected to become a candidate for a higher degree. The main subjects of research, under the general direction of Professor H. Lipson, are X-ray diffraction and optics, and it is hoped shortly to extend the range to certain topics in solid-state physics. Salary: £575 × £25–£600. Superannuation under F.S.S.U. Family Allowances. Forms of application may be obtained from the Registrar, The Manchester College of Science and Technology, Manchester 1, to whom they should be returned by Saturday, March 21, 1959.

HIGH POLYMER RESEARCH

The British Rayon Research Association has a vacancy for a graduate with a first or second class honours degree (preferably in physics) to undertake original research in the field of mechanical properties of high polymers. Commencing salary in the range £650–£1,100 according to age and qualifications. The laboratories are on the fringes of rural Cheshire, within easy reach of pleasant residential districts. Assistance may be given with accommodation. Apply to the Secretary, British Rayon Research Association, Heald Green Laboratories, Wythenshawe, Manchester 22, quoting ref. P.5.

THE UNIVERSITY OF SHEFFIELD

Applications are invited for a POST-DOCTORAL RESEARCH FELLOWSHIP in the department of fuel technology and chemical engineering. The Fellow will be required to direct and carry out research on the laws of smoke formation and elimination with all types of fuel. Initial salary £1,000 a year, with F.S.S.U. provision. Further particulars may be obtained from the Registrar to whom applications should be sent by April 18, 1959.

UNIVERSITY OF READING

DEPARTMENT OF PHYSICS

A vacancy exists for an honours GRADUATE PHYSICIST to work on radiation effects in solids. The department is equipped with a 2 MeV electron accelerator which is being used entirely for work in this field. Applicants should preferably have had two years' research experience but this is not essential. Opportunities exist for reading for higher degrees. The salary will depend on experience. Applications should be submitted to Professor R. W. Ditchburn, Physics Research Laboratory, The University, Upper Redlands Road, Reading.

WELWYN ELECTRICAL LABORATORIES LTD.

Bedlington, Northumberland

RESEARCH PHYSICIST is required by a company engaged in the development of a wide variety of components used by the electronic industry. Well-equipped laboratories exist and an increasing program of basic research and interesting development is in hand. This appointment will provide an opportunity for a qualified candidate to establish himself in a vital and rapidly expanding field in which interest in many branches of physics may be satisfied.

Clearly defined salary scales exist and it is intended that the successfull applicant be placed on a scale in the range £800 – £1,200 per annum with minimum annual increments of £50.

An invitation to inspect the laboratories and to ascertain the amenities provided will be extended to selected candidates who provide full details of their career and qualifications to the Personnel Manager.

NORWOOD TECHNICAL COLLEGE

LECTURER IN PHYSICS required as soon as possible to teach to B.Sc. general standard; facilities in department for approved research. Applicants should have honours degree; research or industrial experience an advantage. Burnham F.E. salary scale: £1,260 × £31 10s. to £1,417 10s. plus London allowance £37 16s. or £50 8s. Application forms from Principal at College, Knights Hill, S.E.27, to be returned by April 10, 1959.

SENIOR SCIENTIFIC OFFICERS (*a*): SCIENTIFIC OFFICERS (*b*). Pensionable posts for men or women in all major scientific fields, including physics, chemistry, biology, meteorology and mathematics. Age limits: (*a*) at least 26 and under 31, (*b*) at least 21 and under 28. Extension for regular Forces Service and Overseas Civil Service. Qualifications: normally first or second class honours degree in science, mathematics or engineering, or equivalent attainment; additionally for (*a*), at least 3 years' relevant (e.g. postgraduate) experience. London salaries (men): (*a*) £1,190–£1,410, (*b*) £635–£1,110; provision for starting pay above minimum. Promotion prospects. Write Civil Service Commission, 17 North Audley Street, London, W.1, for application form, quoting (*a*) S53/59, (*b*) S52/59.

UNIVERSITY OF ST. ANDREWS QUEEN'S COLLEGE, DUNDEE

The Edward A. Deeds Fellowship

Applications are invited for the Edward A. Deeds Fellowship in Queen's College, Dundee, within the University of St. Andrews, for research in engineering, metallurgy, chemistry, physics, mathematical physics or any cognate subject. The Fellowship (which is open to persons of either sex) is of the value of £1,500 per annum, with F.S.S.U. and family allowance benefits. It will be tenable for three years. The Fellow will have the status of a University Lecturer, and may be required, as part of his duties, to do a limited amount of advanced teaching in the department in which he proposes to work. The appointment will take effect from a date to be arranged, and applications are requested to submit ten copies of their application to the undernoted not later than March 31, 1959. Patrick Cumming, Secretary to the Deeds Fellowship Committee.

SENIOR PHYSICIST. The Cementation Company is establishing a central research department in North-West London to further the Group's activities at home and abroad, and needs a Senior Physicist to lead the work of the physical section. The Company undertakes extensive civil and mining contracts, specializing in foundations, subterranean engineering and geophysical prospecting. There are also subsidiaries with products ranging from anti-vibration mounting and hard metals to building chemicals and materials. The physical section will thus handle an extensive range of problems likely to include electrical physics, vibration and wave propagation, flow of fluids and suspensions, geophysical techniques, mechanics of cutting, measurement of stress and strain and radioactivity techniques. Experience in any of these fields will be helpful but prime considerations are sound technical background and imagination coupled with initiative and energy. Candidates should have a good honours degree in physics and be between 28 and 35. The position is a senior one, carrying a substantial salary and car allowance. Assistance can be given with housing. Applications should be sent to: Chief Research Officer, The Cementation Company Ltd., 20 Albert Embankment, London, S.E.11.

BATTERSEA COLLEGE OF TECHNOLOGY LONDON, S.W.11

(A College of Advanced Technology)

Department of Physics

Applications are invited for (*a*) LECTURER in microwave physics. Research experience essential, teaching experience desirable. The microwave section is concerned with research, for which good facilities are provided, and with the teaching of students in their final undergraduate and postgraduate years. Salary, £1,298 × £31 10s.–£1,418, less statutory deductions. (*b*) LECTURER (Grade B) in low temperature physics. Some research or teaching experience essential. At present research is conducted in the properties of metals at low temperatures. Salary £799 × £26 5s.–£1,205, plus good honours allowance of £52 10s. and increments for training and experience, less statutory deductions. Further particulars from the Clerk to the Governing Body by whom applications should be received as soon as possible.

Applications are invited by the department of physics from honours graduates in physics, chemistry or in a biological subject, for STUDENTSHIPS leading to the degrees of Ph.D. or M.Sc. in biophysics (University of London). The aim of these studentships is to provide facilities for basic training in the branches of physical and biological science appropriate to the peaceful uses of atomic energy. The Studentships normally extend over two or three years and carry an allowance of £400 per annum. Detailed applications naming two referees, for places available from October 5, 1959, should be received by the Director, Department of Physics, Institute of Cancer Research, Royal Cancer Hospital, Fulham Road, London, S.W.3, not later than April 20, 1959.

UNIVERSITY OF GLASGOW LECTURESHIPS IN EXPERIMENTAL AND THEORETICAL PHYSICS

Applications are invited for Lectureships in experimental and theoretical physics. Salary scale: Lecturer—£900 – £1,650; Senior Lecturer — £1,550 – £2,000. Grade of appointment and initial salary according to experience and qualifications. F.S.S.U. and family allowance benefits. Successful applicants will be expected to prosecute research preferably in nuclear or elementary particle physics in addition to teaching duties. Applications (8 copies), including the names of two referees, should be lodged not later than April 15, 1959, with the undersigned from whom further particulars may be obtained. Robert T. Hutcheson, Secretary of University Court.

METAL PHYSICS AND AUTOMATION

These may seem far apart, but there are vacancies for GRADUATE STAFF to work in both these fields of research in the Physics Department of the British Iron and Steel Research Association in London. We should like to hear from you if you are a graduate (preferably honours) in physics, metallurgy or electrical engineering and are keen to do research in either of these subjects. Relevant research experience would be an advantage, but we regard as essentials an enthusiasm and ability to generate and properly develop new ideas. Starting salary will be offered which is commensurate with experience and qualifications, and there are opportunities for quick advancement.
Written applications only quoting "M.P.4" to the Personnel Officer, B.I.S.R.A., 11 Park Lane, W.1.

TECHNICAL SALES MANAGER

AMPLIVOX LIMITED, the leading European manufacturer of audiometers and speech training equipment, have a vacancy for the senior position of Manager of their Medical Acoustic Division. Applications are invited from men aged 30–45 who are medical or scientific graduates or who have other technical qualifications and who will preferably have had sales experience. The position calls for a man with ability to sell technical products to the medical profession, hospitals and local authorities, to train personnel and to write technical reports. Depending upon qualifications, a salary of up to £1,500 a year is offered, a company car will be provided and the post is pensionable. Write with full details to:—General Sales Manager, Amplivox Limited, Beresford Avenue, Wembley, Middlesex.

EDWARDS HIGH VACUUM LTD., Manor Royal, Crawley, Sussex, invite applications for the following posts in their Research Laboratories.

(*a*) RESEARCH PHYSICIST OR MECHANICAL ENGINEER, to investigate problems connected with obtaining very high vacua in large scale apparatus. Applicants should hold a degree, and have had several years' postgraduate experience.

(*b*) RESEARCH PHYSICIST OR ELECTRICAL ENGINEER, to work on the research and development of instrument measuring and control systems. A degree or equivalent qualification is essential for the vacancy and electronics or electron physics experience would be an advantage.

(*c*) RESEARCH ASSISTANT, to assist in the investigations of melting techniques by electronic bombardment. Applicants (age 21-25) should have a pass degree or a Higher National Certificate in Applied Physics. Some previous experience and a knowledge of electronics would be an advantage.

Commencing salaries will be in accordance with age and experience. Pensions and bonus schemes are operated. Housing available if from London Area. Write giving full details to the Personnel Manager.

PHYSICIST for development work on Ammonia Masers. Original work is now needed and provides an outstanding opportunity for a man with ability and drive. A good degree plus experience in vacuum physics and instrument development is essential. Salary at least £1,200 p.a. Write giving particulars to Glass Developments Ltd., Sudbourne Road, S.W.2.

RESEARCH ENGINEER/PHYSICIST
DIVISION OF METROLOGY, C.S.I.R.O., AUSTRALIA

Applications are invited from honours graduates in engineering or science, with physics as major subject and postgraduate experience. Must be capable of initiating and developing new and improved measuring techniques and equipment in engineering metrology, particularly large-scale metrology, dynamic metrology and gears and cutting equipment. Salary dependent on qualifications and experience in the ranges: £A1,326–1951 p.a. or £A2,061–2,336 p.a. Further particulars of conditions, fares, etc., obtainable from: Mr. E. J. Drake, Chief Scientific Liaison Officer, Africa House, Kingsway, London, W.C.2, to whom applications, referring to appointment No. 750/188, should be addressed by March 28, 1959.

AUSTRALIA—
UNIVERSITY OF QUEENSLAND
PROFESSOR OF APPLIED MATHEMATICS

Applications are invited for the above position. Applicants should have had experience of teaching at university level and have made significant original contributions to some branch or branches of applied mathematics.
Salary £A3,500 p.a.
Further particulars and application forms are obtainable from the Secretary, Association of Universities of the British Commonwealth, 36 Gordon Square, London, W.C.1 or from the Registrar, University of Queensland, Brisbane, Queensland, Australia.
Applications close, in Australia and London, on April 30, 1959.

APPOINTMENTS REQUIRED

Associate aged 41 with experience in optical design, technical teaching, meteorology, atmospheric hydrodynamics, statistics and administration requires post. Willing to specialize in any physically based subject provided prospects are good. Available because of political changes in the Colonial Empire. Communications should be sent to Box 1, The Institute of Physics, 47 Belgrave Square, London, S.W.1.

Fellow, retired from university teaching on age grounds only but still mentally and physically alert and with exceptionally good record, offers his services in any capacity. Particularly interested in lecturing or organizing laboratory work in "Introductory" or Intermediate Physics —also in Applied Mathematics. Hoping for relatively permanent post but willing to deputise for lecturers absent on sick or sabbatical leave if a program for some three or four years can be arranged in advance. Communications should be sent to Box 2, The Institute of Physics, 47 Belgrave Square, London, S.W.1.

COURSES

UNIVERSITY OF CAMBRIDGE
DEPARTMENT OF ENGINEERING
POSTGRADUATESHIP COURSE IN CONTROL ENGINEERING

The 1959-60 postgraduate course in control engineering will begin on October 6, 1959, and will last approximately nine months. This is the fifth course of a series which has been established in order to meet the need of industry for men trained to a high standard in theoretical principles and practical application of automatic control and to afford opportunity for study of advanced techniques and modern development in the general field of control engineering.
The number of students will be limited and applications must reach the Secretary of the Department not later than July 1, 1959. Candidates should hold a degree in engineering or suitable alternative qualification and have had some practical training and experience. They will be required to attend for interview in Cambridge on or before August 15, 1959. Further details and forms of application for admission may be obtained from the Secretary, Cambridge University Department of Engineering, Trumpington Street, Cambridge.

BIRKBECK COLLEGE
(University of London)
Session 1959-60 begins Monday, October 5, 1959

Part-time (evening) courses provided for Internal degrees in the Faculties of Arts and Science. Facilities also provided for full-time and part-time students reading for
(i) Higher degrees by thesis in Arts and Science,
(ii) M.A. and M.Sc. Mathematics and M.Sc. Crystallography by examination.
(iii) Academic postgraduate Diplomas in Psychology and Numerical Analysis.

The College also invites applications from full-time and part-time students wishing to take by thesis higher degrees in Statistics.
The Department of Scientific and Industrial Research has accepted the Postgraduate Diploma courses in Psychology and Numerical Analysis, and the M.Sc. Crystallography course, as suitable for tenure of its Advanced Course Studentships. Students intending to read for Higher Degrees in Science by thesis are invited to enquire of the College whether it has available any D.S.I.R. Research Studentships. Applications for admission to the College should be made before June 1. Pamphlet and form of application may be obtained from the Registrar, Birkbeck College, Malet Street, W.C.1.

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